DOCUMENT RESUME

ED 457 358 CE 082 385

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TITLE National Job Corps Study: The Impacts of Job Corps on Participants' Employment and Related Outcomes [and] Methodological Appendixes on the Impact Analysis.
INSTITUTION Mathematica Policy Research, Washington, DC.; Battelle Memorial Inst., Seattle, WA.; Decision Information Resources, Inc., Houston, TX.
SPONS AGENCY Employment and Training Administration (DOL), Washington, DC. Office of Policy and Research.
PUB DATE 2001-06-00
NOTE 612p.; MPR Reference No. 8140-530. For a previous Job Corps Study, see ED 448 345.
CONTRACT K-42793-00-80-30
AVAILABLE FROM For full text: http://wdr.doleta.gov/opr/fulltext/document.asp?docn=6155 (Report) and http://wdr.doleta.gov/opr/fulltext/document.asp?docn=6156 (Appendixes).
PUB TYPE Reports - Evaluative (142)
EDRS PRICE MF03/PC25 Plus Postage.
DESCRIPTORS Adolescents; Cost Effectiveness; Crime; Crime Prevention; *Disadvantaged; *Employment Programs; Evaluation Methods; *Federal Programs; Job Placement; *Job Training; Juvenile Courts; *Outcomes of Education; *Program Effectiveness; Program Evaluation; Research Methodology; Validated Programs; Young Adults; Youth Employment
IDENTIFIERS Impact Studies; *Job Corps

ABSTRACT

A study involving random assignment of all youth eligible for Job Corps to either a Job Corps program or to a control group was conducted to assess the impact of Job Corps on key participant outcomes. Participants in the study were nationwide youth eligible for Job Corps who applied for enrollment for the first time between November 16, 1994, and December 17, 1995. The study sought to determine the following:(1) how effectively Job Corps improves the employability of disadvantaged participants, (2) whether Job Corps impacts differ for youths with different baseline characteristics, and (3) how effective the residential and nonresidential components of Job Corp are. Findings over the first 4 years after random assignment include the following: (1) Job Corps provided extensive education, training, and other services to the program group and improved their educational attainment; (2) Job Corps generated positive employment and earnings impacts by the beginning of the third year after random assignment and the impacts persisted through the fourth year; (3) employment and earnings gains were found broadly across most subgroups of students; (4) the resident and nonresidential programs were each effective for the youths they served; (5) Job Corps significantly reduced youths' involvement with the criminal justice system; (6) Job Corps had small beneficial impacts on the receipt of public assistance and self-assessed health status, but no impacts on illegal drug use; and (7) Job Corps had no impacts on fertility or custodial responsibility, but it slightly promoted independent living and mobility. (The report include numerous tables and

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charts, 31 references, and five appendixes concerning the study methodology.) (KC)

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ED 457 358

Contract No.: K-4279-3-00-80-30
MPR Reference No.: 8140-530

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# National Job Corps Study: The Impacts of Job Corps on Participants' Employment and Related Outcomes

*June 2001*

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CE082385

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# ACKNOWLEDGMENTS

We would like to thank the many people whose efforts have made this report possible. These include those involved in the design and implementation of random assignment, the collection of survey data, and the preparation of the document itself.

The study design was developed by a team that included Charles Metcalf, Sheena McConnell, and John Homrighausen from Mathematica Policy Research, Inc. (MPR), Terry Johnson from Battelle Human Affairs Research Centers, Mark Gritz from the Sphere Institute, Russell Jackson from Decision Information Resources, Inc. (DIR), and the first two authors of this report. The operational design and study implementation benefited greatly from the contributions of many people at the U.S. Department of Labor (DOL): Daniel Ryan, project officer for the study; Karen Greene; David Lah; Peter Rell, Job Corps Director during the period of design and early implementation; Mary Silva, Job Corps Director during the period covering the previous impact report; Richard Trigg, current Job Corps Director; Alexandra Kielty; Jenny Gallo; Brian Kennedy; Edna Primrose-Coates; Jim Woods; and the regional Job Corps Directors and regional office study coordinators in each of the nine Job Corps regions. Members of the study advisory panel also made important contributions to the design and focus of the study. In addition, John Homrighausen, Marianne Stevenson, Linda Gentzik, and Mike Watts at MPR designed and supervised the processing of information from more than 100,000 youths nationwide. We would especially like to recognize the efforts and contributions of the hundreds of Job Corps outreach and admissions counselors nationwide, who explained the study to new Job Corps applicants.

Many of these same people also made significant contributions to the content and structure of this report.

The impact analysis would not have been possible without the efforts of the many people who conducted several rounds of interviews with a large nationwide sample of mobile youths over a four-year period. John Homrighausen served as survey director throughout; Cindy Steenstra supervised telephone center interviewing and searching operations; Donna Kratzer and Bill Beecroft managed in-person interviewing for the 30- and 48-month data collection effort; Barbara Rogers performed this role on the 12-month data collection; and Sharon De Leon and Edward Freeland did so for the baseline data collection. DIR conducted in-person interviewing in the South and Southwest for all rounds of data collection. Key DIR staff were Pamela Wells and Eleanor Tongee. Todd Ensor and John Homrighausen developed the survey instruments and oversaw preparation of the CATI programs to support telephone interviewing. Mike Watts managed sample release and provided support for CATI operations and reporting. Ben Shen assisted in providing support for the data system necessary to manage field interviewing. We are grateful to the many telephone and field interviewers who were involved in the data collection effort, and finally, to the young men and women in the sample who patiently answered our many questions.

Jeanne Bellotti, Ruo-Jiao Cao, April Grady, and Melissa Seeley provided excellent programming assistance throughout the course of the study; they constructed the data files, wrote the subroutines to produce the impact estimates, and prepared the tabulations. Steve Bishop took the lead in the arduous task of hand coding much of the data on arrests. Walter Brower and Patricia Ciaccio provided valuable editorial assistance. Cathy Harper did an excellent job of producing this report, with assistance from Monica Capizzi, Jill Miller, Jennifer Chiaramonti, Cindy McClure, and Jane Nelson.

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# ABSTRACT OF FINDINGS

The Job Corps program has long been a central part of federal efforts to provide training for disadvantaged youths. Because of the high costs of the program's intensive services, which are provided mainly in a residential setting, policymakers need to know just how effective Job Corps actually is. This report presents the findings of the National Job Corps Study on impacts of the program on participants' employment and related outcomes.

The cornerstone of the National Job Corps Study was the random assignment of all youths found eligible for Job Corps to either a program group or a control group. Program group members could enroll in Job Corps; control group members could not, but they could enroll in all other programs available to them in their communities. We estimated impacts by using data from periodic follow-up interviews to compare the experiences of the program and control groups. Findings on program impacts over the first four years after random assignment are summarized below.

***Job Corps provided extensive education, training, and other services to the program group.*** Follow-up interviews show that 73 percent of the program group enrolled in Job Corps, with an average period of participation of eight months. Students received large amounts of academic classroom instruction and vocational skills training. They also participated extensively in the primary Job Corps activities outside the classroom.

***Job Corps substantially increased the education and training services that eligible applicants received, and it improved their educational attainment.*** On average, Job Corps increased participants' time spent in education and training (both in and out of Job Corps) by about 1,000 hours, approximately the number in a regular 10-month school year. It also focused more on vocational instruction than did the training available elsewhere. Job Corps substantially increased the receipt of GED and vocational certificates, but it had no effect on college attendance.

***Job Corps generated positive employment and earnings impacts by the beginning of the third year after random assignment, and the impacts persisted through the end of the 48-month follow-up period.*** During the last year of the 48-month follow-up period, the gain in average earnings per participant was about $1,150, or 12 percent. Over the entire period, Job Corps participants earned about $624 more than they would have if they had not enrolled in Job Corps.

***Employment and earnings gains were found broadly across most subgroups of students.*** Employment-related impact estimates were similar for males and females. Earnings gains were found for groups of students at special risk of poor outcomes (such as very young students, females with children, and older students without a high school credential at enrollment), as well as for groups at lower risk (such as older students with a high school credential).

***The residential and nonresidential programs were each effective for the youths they served.*** Postprogram earnings and employment impacts for those assigned to each component were positive overall, and for nearly all groups defined by gender and the presence of children. The beneficial impacts for nonresidential females with children are noteworthy, because they suggest that the nonresidential program allows Job Corps to serve effectively a group that, because of family responsibilities, would otherwise be unable to participate.

***Job Corps significantly reduced youths' involvement with the criminal justice system.*** The arrest rate was reduced by 16 percent (about 5 percentage points). Arrest rate reductions were largest during the first year after random assignment (when most program enrollees were in Job Corps), although Job Corps also led to small reductions during the later months of the follow-up period. Reductions occurred for nearly all categories of crimes, although they were slightly larger for less serious ones. The impacts on arrest rates were very similar across subgroups. Job Corps participation also reduced convictions and incarcerations resulting from a conviction by about 17 percent. Finally, Job Corps led to reductions in crimes committed against program participants.

***Job Corps had small beneficial impacts on the receipt of public assistance and on self-assessed health status, but it had no impacts on illegal drug use.*** Overall, program group members reported receiving about $460 less in benefits (across several public assistance programs) than control group members. Program group members were slightly less likely than control group members to report their health as "poor" or "fair"--15.5 percent, compared to 17.5 percent at each interview point. There were no differences in the reported use of alcohol and illegal drugs or in the use of drug treatment services.

***Job Corps had no impacts on fertility or custodial responsibility, but it slightly promoted independent living and mobility.*** Participation in Job Corps had no impacts on having a child or on the likelihood of living with or providing support for a child. However, a slightly smaller percentage of program group than control group members were living with their parents, and a slightly larger percentage (31 percent, compared to 29 percent) were living with a partner either married or unmarried. The average distance between the zip codes of residence at program application and at 48 months was slightly larger for the program group. However, because most students returned to their home communities, Job Corps had no effect on the characteristics of the places in which the youths lived.

In conclusion, we find that Job Corps produces beneficial impacts on the main outcomes that it intends to influence. Beneficial impacts on education-related, employment-related, and crime-related outcomes were found overall, as well as for broad subgroups of students in the program. The residential and nonresidential program components were each effective for the students they served. A companion report, which presents findings from the benefit-cost analysis, concludes that Job Corps is a worthwhile investment both for the students and for the broader society that supports their efforts.

# EXECUTIVE SUMMARY

Since 1964, the Job Corps program has been a central part of federal efforts to provide employment assistance to disadvantaged youths between the ages of 16 and 24. It is an intensive, comprehensive program whose major service components include academic education, vocational training, residential living, health care and health education, counseling, and job placement assistance. These services are currently delivered at 119 Job Corps centers nationwide. Most Job Corps students reside at Job Corps centers while training, although about 12 percent are nonresidential students who live at home. Each year, Job Corps serves more than 60,000 new enrollees and costs more than $1 billion.

The National Job Corps Study, funded by the U.S. Department of Labor (DOL), was designed to provide a thorough and rigorous assessment of the impacts of Job Corps on key participant outcomes. The cornerstone of the study was the random assignment of all youth found eligible for Job Corps to either a program group or a control group. Program group members were allowed to enroll in Job Corps; control group members were not (although they could enroll in other training or education programs).

This report presents estimates of the impacts of Job Corps on participants' employment and related outcomes during the 48 months after random assignment. The outcome measures for the analysis were obtained from interview data.

The report answers the following three research questions:

1. ***How effective is Job Corps overall at improving the employability of disadvantaged participants?*** Job Corps participation led to (1) increases of about 1,000 hours (or about one school year) in time spent in education and training; (2) substantial increases in the attainment of GED and vocational certificates; (3) earnings gains by the beginning of the third year after random assignment that persisted through the end of the follow-up period (resulting in a 12 percent gain in year 4); (4) reductions of about 16 percent in arrests, convictions, and incarcerations for convictions; (5) reductions in crimes committed against participants; (6) small beneficial impacts on the receipt of public assistance and self-assessed health status; (7) small increases in the likelihood of living with a partner and living independently; (8) no impacts on self-reported alcohol and illegal drug use, fertility, or custodial responsibility, but some increases in the use of child care.

2. ***Do Job Corps impacts differ for youths with different baseline characteristics?*** Job Corps is effective for broad groups of students. Program participation led to substantial improvements in education-related outcomes across diverse groups of students. Employment and earnings gains were similar for males and females, and were found for groups of students at special risk of poor outcomes (such as very young students, females with children, and older students without a high school credential at

enrollment), *as well as* for groups at lower risk (such as older students with a high school credential). Reductions in criminal activity were found for nearly all groups.

3. ***How effective are the residential and nonresidential components of Job Corps?*** Each component is effective for the groups it serves. Postprogram earnings and employment impacts for those assigned to each component were positive overall, and for nearly all groups defined by gender and the presence of children. Participation in each component led to reductions in criminal activity for most groups of students, except that no reductions were found for nonresidential males.

A separate report presents findings from the benefit-cost analysis (McConnell et al. 2001), where program benefits (calculated by placing a dollar value on the estimated program impacts) are compared to program costs. That report concludes that the benefits of Job Corps exceed the substantial public resources that are invested in it.

## STUDY DESIGN

The results for the impact analysis are based on a comparison of eligible program applicants who were randomly assigned to a program group (who were offered the chance to enroll in Job Corps) or to a control group (who were not). The key features of this experimental design are as follows:

**The impact evaluation is based on a fully national sample of eligible Job Corps applicants.** With a few exceptions, the members of the program and control groups were randomly selected from *all* youths who applied to Job Corps in the contiguous 48 states and the District of Columbia and who were found eligible for the program.

**Sample intake occurred between November 1994 and February 1996.** All youths who applied to Job Corps for the first time between November 1994 and December 1995 and were found eligible for the program by the end of February 1996 were included in the study--a total of 80,883 eligible applicants.

**During the sample intake period, 5,977 Job Corps-eligible applicants were randomly selected to the control group.** Approximately 1 eligible applicant in 14 (7 percent of 80,883 eligible applicants) was assigned to the control group. For both programmatic and research reasons, the sampling rate to the control group differed somewhat across some youth subgroups. Thus, sample weights were used in all analyses, so that the impact estimates could be generalized to the intended study population.

**Control group members were not permitted to enroll in Job Corps for a period of three years, although they were able to enroll in other programs available to them.** Thus, the outcomes of the control group represent the outcomes that the program group would have experienced if they had not been given the opportunity to enroll in Job Corps. Because control group members were allowed to enroll in other education and training programs, the comparisons of program and control group outcomes represent the effects of Job Corps *relative to other available programs* that the study population would enroll in if Job Corps were not an option. The impact estimates do not represent the effect of the program relative to no education or training; instead, they represent the incremental effect of Job Corps.

**During the sample intake period, 9,409 eligible applicants were randomly selected to the research sample as members of the program group.**[1] Because random assignment occurred after youths were determined eligible for Job Corps (and *not* after they enrolled in Job Corps centers), the program group includes youths who enrolled in Job Corps (about 73 percent of eligible applicants), as well as those who did not enroll, the so-called "no-shows" (about 27 percent of eligible applicants). Although the study's research interest focuses on enrollees, all youths who were randomly assigned, including those who did not enroll at a center, were included in the analysis to preserve the benefits of the random assignment design. However, as discussed below, statistical procedures were also used to estimate impacts for Job Corps participants only.

**Job Corps staff implemented random assignment procedures well.** Using program data on all new center enrollees, we estimate that less than 0.6 percent of youths in the study population were not randomly assigned. In addition, only 1.4 percent of control group members enrolled in Job Corps before the end of the three-year period during which they were not supposed to enroll.[2] Hence, we believe that the research sample is representative of the youths in the intended study population and that the bias in the impact estimates due to contamination of the control group is very small.

## DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS

The impact analysis used a variety of data sources, outcome measures, and analytic methods to address the main study questions, as outlined next.

**The analysis relied primarily on interview data covering the 48-month period after random assignment.** Follow-up interview data collected 12, 30, and 48 months after random assignment were used to construct outcome measures for the impact analysis. In addition, baseline interview data, collected soon after random assignment, were used to create subgroups defined by youth characteristics at random assignment, and to construct outcome measures that pertain to the period between the random assignment and baseline interview dates.

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[1] The remaining 65,497 eligible applicants were randomly assigned to a program nonresearch group. These youths were allowed to enroll in Job Corps but are not in the research sample.

[2] An additional 3.2 percent of control group members enrolled in Job Corps after their three-year restriction period ended and before four years after random assignment.

**Response rates to the baseline, 12-month, 30-month, and 48-month interviews were fairly high and were similar for program and control group members.** The response rate was 95 percent to the baseline interview, 90 percent to the 12-month follow-up interview, 79 percent to the 30-month interview, and 80 percent to the 48-month interview. Response rates were similar across key subgroups.

**The primary sample used for the analysis includes those who completed 48-month interviews.** This sample contains 11,313 youths (6,828 program group members and 4,485 control group members). About 88 percent of this sample also completed 30-month interviews, and 95 percent completed 12-month interviews. Furthermore, baseline interview data are available for everyone in this sample, because all youths completed either the full baseline interview or an abbreviated baseline interview in conjunction with the 12-month interview. Thus, complete data are available for most of the analysis sample.

**The study estimated impacts on the following outcome measures that we hypothesized could be influenced by participation in Job Corps: (1) education and training, (2) employment and earnings, and (3) nonlabor market outcomes.** The nonlabor market outcomes include welfare, crime, alcohol and illegal drug use, health, family formation, child care, and mobility. In general, outcome measures were defined over several periods after random assignment. We constructed measures by quarter (to examine changes in impact estimates over time), for year 1 (a period when many program group members were enrolled in Job Corps), for year 2 (a period of still significant but less intensive Job Corps participation), for years 3 and 4 each (a postprogram period for most program group members), and for the entire 48-month period.

**We present estimates of Job Corps impacts per eligible applicant and per Job Corps participant.** The estimates of Job Corps impacts *per eligible applicant* were obtained by computing differences in the distribution of outcomes between all program and control group members. This approach yields unbiased estimates of the effect of Job Corps for those offered the opportunity to enroll in the program. These impacts are pure experimental estimates, because random assignment was performed at the point that applicants were determined to be eligible for the program.

The comparison of the outcomes of all program and control group members yields *combined* impact estimates for the 73 percent of program group members who enrolled in Job Corps centers and the 27 percent who did not. Policymakers, however, are more concerned with the effect of Job Corps on those who enrolled in a center and received Job Corps services. This analysis is complicated by the fact that we do not know which control group members would have shown up at a center had they been in the program group. However, this complication can be overcome if we assume that Job Corps has no impact on eligible applicants who do not enroll in centers. In this case, the impact *per participant* can be obtained by dividing the impact *per eligible applicant* by the proportion of program group members who enrolled in Job Corps (73 percent).[3] We present estimated impacts both per eligible applicant and per participant.

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[3] The estimates per participant were further refined to adjust for the small number of control group members who enrolled in Job Corps during their three-year restriction period, by dividing the impacts per eligible applicant by the difference between the participation rate among the program group and the control group crossover rate.

**Impact estimates were obtained for key subgroups defined by youth characteristics at baseline.** The purpose of this subgroup analysis was to identify groups of Job Corps students who benefit from program participation and those who do not, so that policymakers can improve program services and target them appropriately. We estimated impacts of Job Corps on the following seven sets of subgroups: (1) gender, (2) age at application to Job Corps, (3) educational attainment, (4) presence of children for females, (5) arrest experience, (6) race and ethnicity, and (7) whether the youth applied to the program before or after new zero tolerance (ZT) policies took effect.[4] Subgroup impact estimates were obtained by comparing the distribution of outcomes of program and control group members in that subgroup. For example, impacts for females were computed by comparing the outcomes of females in the program and control groups.

**We estimated separate impacts for those assigned to the residential and nonresidential program components.** These impacts were estimated using data on the predictions of outreach and admission (OA) counselors as to whether sample members would be assigned to a residential or a nonresidential slot. As part of the application process, OA counselors filled in this information on a special form developed for the study. The anticipated residential status information is available for both program *and* control group members, because it was collected prior to random assignment. Thus, the impacts of the residential component were estimated by comparing the distribution of outcomes of program group members designated for a residential slot with those of control group members designated for a residential slot. Similarly, the impacts of the nonresidential component were estimated by comparing the experiences of program and control group members designated for nonresidential slots. This analysis produced reliable estimates of program impacts for residential and nonresidential students, because the anticipated residential status information is available for all sample members, and because it matched actual residential status very closely for program group members who enrolled in Job Corps.

An important point about the interpretation of the impact findings for residents is that they tell us about the effectiveness of the residential component *for youths who are typically assigned to residential slots*. Similarly, the impact estimates for nonresidents tell us about the effectiveness of the nonresidential component *for youths who are typically assigned to nonresidential slots*. The characteristics of residential and nonresidential students differ (nonresidential students tend to be females with children and tend to be older). Consequently, our results cannot necessarily be used to measure the effectiveness of each component for the *average* Job Corps student. Nor can they be used to assess how a youth in one component would fare in the other one.

## JOB CORPS EXPERIENCES

Job Corps staff have implemented a well-developed program model throughout the country (as described in a separate process analysis report by Johnson et al. [1999]). To understand the impacts that Job Corps had on the employment and related outcomes of participants, we must examine the

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[4] In response to congressional concerns about the operation of the Job Corps program, and in particular, about safety on center, new ZT policies for violence and drugs were instituted in March 1995--during the sample intake period for the study. The new policies were instituted to ensure full and consistent implementation of existing policies for violence and drugs.

Job Corps experiences of the program group. Because we can expect meaningful Job Corps impacts on key outcomes only if program group members received substantial amounts of Job Corps services, we examined whether program group members received services, and then gauged the intensity and types of those services.

Our results, which indicate that program group members received extensive Job Corps services, can be summarized as follows:

**Most program group members enrolled in Job Corps.** Of those assigned to the program group, 73 percent reported enrolling in Job Corps within 48 months.

**Participants typically enrolled very soon after random assignment.** The average enrollee waited 1.4 months, or about six weeks, to be enrolled in a Job Corps center, although nearly three-quarters of those who enrolled did so in the first month, and only four percent enrolled more than six months after random assignment.

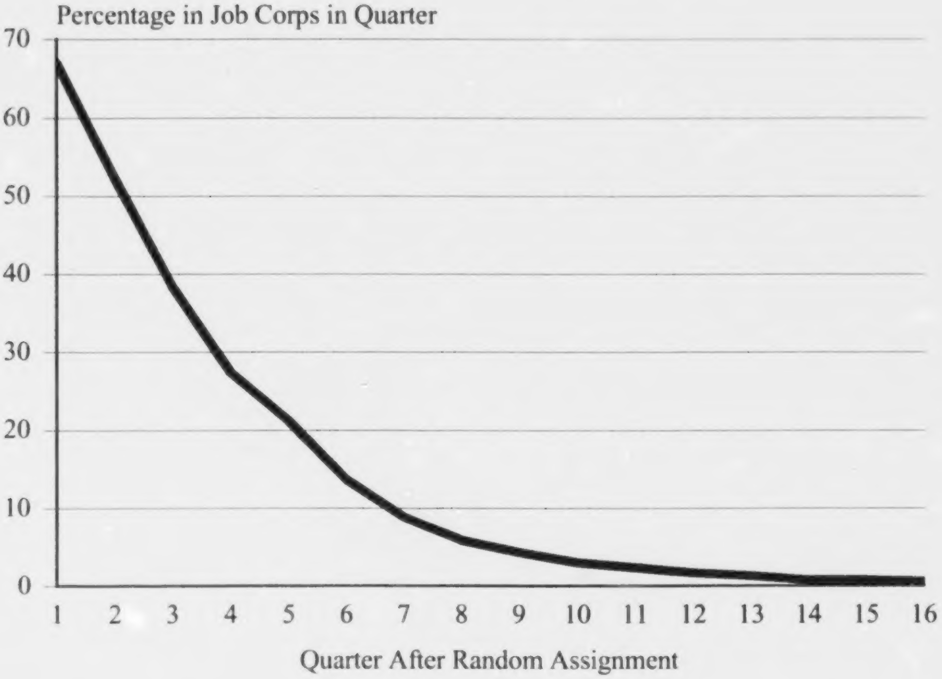
**Most participants stayed in Job Corps for a substantial period of time, although the period of participation varied considerably.** The average period of participation per enrollee was eight months. About 28 percent of all enrollees participated less than three months, and nearly a quarter participated for over a year. Because of this wide range in the duration of stay in Job Corps, participants left Job Corps at different points during the follow-up period.

**The average postprogram period for participants was more than three years.** Variations in the duration of participation in Job Corps resulted in variations in how much of the 48-month period was actually a postprogram period. However, most participants had been out of Job Corps for some time at the 48-month point: almost 67 percent of enrollees had been out for more than three years, and nearly 92 percent for more than two years. Less than 3 percent of enrollees had been out for less than one year.

**Most participation occurred during the first 24 months after random assignment; the final two years of the 48-month period was a postprogram period for most participants (Figure 1).** Figure 1 shows the fraction of program group members (including the no-shows) who participated in Job Corps during each quarter after random assignment. The participation rate declined from a peak of 67 percent in the first quarter after random assignment to 21 percent in the fifth quarter (beginning of the second year), and 3 percent in the tenth quarter. By the end of the 48-month period, almost all participants had left Job Corps. Only 0.3 percent of the program group (0.4 percent of enrollees) were in Job Corps in the final week of the 48-month follow-up period.

FIGURE 1

JOB CORPS PARTICIPATION RATES FOR THE FULL PROGRAM GROUP, BY QUARTER



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

Based on these broad patterns of participation, we interpret the period from quarters 1 to 4 (year 1) as largely an "in-program" period. The period from quarters 5 to 8 (year 2) was a period of transition, in which smaller yet still substantial fractions of the program group were engaged in Job Corps training. The final eight quarters (years 3 and 4) were a postprogram period for most students. The use of these in-program, transition, and postprogram periods provides a framework to help explain the time profiles of employment and earnings and related impacts.

**Program group enrollees participated extensively in the core Job Corps activities.** As the program design intends, a large majority of Job Corps participants (77 percent) received both academic instruction and vocational training. More than 82 percent of enrollees reported receiving academic instruction, and nearly 89 percent received vocational training. The average enrollee reported receiving 1,140 hours of academic and vocational instruction (which is approximately equivalent to one year of classroom instruction in high school). Also, most enrollees participated in the many socialization activities in Job Corps, such as parenting education, health education, social skills training, and cultural awareness classes. Many enrollees, however, reported that they did not receive job placement assistance from the program.

**While many subgroups had different experiences in Job Corps, the differences were small.** The mix of academic and vocational training a student received depended on whether the youth had already received a high school credential (GED or diploma) before program entry. Students with no credential generally took both academic instruction and vocational training. High school graduates were more likely to focus on vocational training. Nonresidential students (especially females with children) had somewhat lower enrollment rates than residential students. Once in Job Corps, however, the residential and nonresidential students had similar amounts, types, and intensity of training, as well as similar exposure to the other program components. The many other subgroup differences were small, and overall each group's experience was consistent with the conclusions drawn above for the program group as a whole.

## EDUCATION AND TRAINING

Job Corps provides intensive academic classroom instruction and vocational skills training to increase the productivity and, hence, the future earnings, of program participants. The typical Job Corps student stays in the program for an extended period (about eight months on average), and Job Corps serves primarily students without a high school credential (about 80 percent of students do not have a GED or high school diploma at program entry). Thus, participation in Job Corps probably increases the amount of education and training participants receive and improves their educational levels relative to what they would have been otherwise.

Important elements of the impact analysis are to describe the education and training experiences of program and control group members and to provide estimates of the impact of Job Corps on key education and training outcomes during the 48 months after random assignment. We examine education and training experiences of the *program group,* both in Job Corps and elsewhere, to provide a complete picture of the services they received. The education and training experiences of the *control group* are the counterfactual for the study, showing what education and training the program group would have engaged in had Job Corps not been available. The net increase in education and training due to Job Corps depends critically on what education and training the control group received and what education and training the program group received from other sources, as well as from Job Corps.

Our main findings can be summarized as follows:

**Many control group members received substantial amounts of education and training.** Nearly 72 percent participated in an education or training program during the 48 months after random assignment. On average, they received 853 hours of education and training, roughly equivalent to three-quarters of a year of high school. Participation rates were highest in programs that substitute for Job Corps: GED programs (37 percent); high school (32 percent); and vocational, technical, or trade schools (29 percent).[5] These high participation rates are not surprising, because control group members demonstrated motivation to go to Job Corps, and thus had the motivation to find other programs.

It is noteworthy that although high school participation rates were high, those who returned to high school stayed there for an average of only about nine months. Because the typical sample member without a high school credential at random assignment had completed less than grade 10, very few control group members graduated from high school.

**Job Corps substantially increased the education and training that program participants received, despite the activity of the control group (Tables 1 and 2).** Nearly 93 percent of the program group engaged in some education or training (both in and out of Job Corps), compared to about 72 percent of the control group (an impact of 21 percentage points per eligible applicant). Job Corps participants spent about 4.8 hours per week--998 hours in total--more in programs than they would have if they had not enrolled in the program. This impact per participant corresponds to *roughly one school year*.

The program group also spent significantly more time in academic classes, and even more in vocational training (Table 2). Program group members spent an average of 3.1 hours per week in academic classes, as compared to 2.5 hours per week for the control group. The program group typically received about three times more vocational training than the control group (3.1 hours per week, compared to 0.9 hours per week).

**The impacts on participation in education and training programs were concentrated in the first six quarters (that is, 18 months) after random assignment (Figure 2).** Impacts were large during this period, because many program group members were enrolled in Job Corps then, but decreased as program group members started leaving Job Corps. About 76 percent of program group members were ever enrolled in an education or training program (including Job Corps and other programs) during the first quarter after random assignment, compared to 29 percent of control group members--an impact per eligible applicant of 47 percentage points. The impact on the participation rate decreased to 22 percentage points in quarter 3 and 10 percentage points in quarter

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[5] The participation rates in GED programs and high school pertain to those who did not have a GED or high school diploma at random assignment.

## TABLE 1

## IMPACTS ON PARTICIPATION AND TIME SPENT IN EDUCATION AND TRAINING PROGRAMS

| | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Estimated Impact per Participant[b] |
|---|---|---|---|---|
| Percentage Ever Enrolled in an Education or Training Program During the 48 Months After Random Assignment | 92.5 | 71.7 | 20.8* | 28.9* |
| Average Percentage of Weeks Ever in Education or Training | 24.4 | 18.2 | 6.3* | 8.7* |
| Average Hours per Week Ever in Education or Training | 7.6 | 4.1 | 3.5* | 4.8* |
| **Sample Size** | **6,828** | **4,485** | **11,313** | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period.

*Significantly different from zero at the .05 level, two-tailed test.

## TABLE 2

## IMPACTS ON PARTICIPATION AND TIME SPENT IN ACADEMIC CLASSES AND VOCATIONAL TRAINING

| | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Estimated Impact per Participant[b] |
|---|---|---|---|---|
| Percentage Ever Took Academic Classes During the 48 Months After Random Assignment | 80.8 | 57.2 | 23.7* | 32.9* |
| Average Hours per Week Ever in Academic Classes | 3.1 | 2.5 | 0.6* | 0.8* |
| Percentage Ever Took Vocational Training | 74.0 | 28.4 | 45.6* | 63.4* |
| Average Hours per Week Ever Received Vocational Training | 3.1 | 0.9 | 2.2* | 3.1* |
| **Sample Size[c]** | **3,383** | **2,350** | **5,733** | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

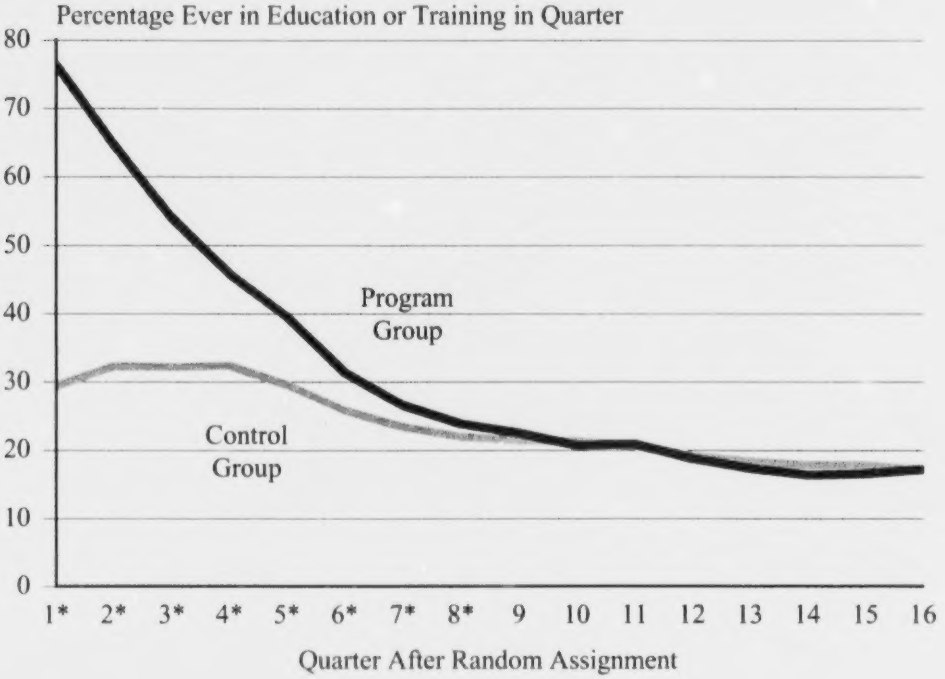
[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period.

[c] The sample consists of those in the 48-month sample (1) who completed a 30-month interview after April 1998, because of an error in the 30-month interview's skip logic before then; and (2) who did not complete a 30-month interview.

*Significantly different from zero at the .05 level, two-tailed test.

## FIGURE 2

## PARTICIPATION RATES IN EDUCATION AND TRAINING PROGRAMS, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

5. The impact was about 3 percentage points in quarter 7 and near zero in each quarter in years 3 and 4.

**Similar percentages of program and control group members were enrolled in education and training programs toward the end of the 48-month period.** For example, about 13 percent of both research groups were enrolled in a program during the last week of the 48-month follow-up period. This finding is important, because it suggests that impacts on employment and earnings late in the 48-month period were not affected by differences in school enrollment rates by research status.

**Control group members spent more time than program group members in programs other than Job Corps, although the differences were smaller than anticipated (Figure 3).** About 71 percent of control group members enrolled in a program other than Job Corps during the 48-month period, compared to 63 percent of program group members. The differences in participation rates in programs that substitute for Job Corps (high school, GED programs, vocational schools, and ABE and ESL programs) are statistically significant. There were no differences in enrollment rates in two- or four-year colleges.[6]

While impacts on participation in alternative programs are statistically significant, they were smaller than expected. Program group members made considerable use of these same programs, which increased impacts on education and training and reduced the offset to Job Corps program costs.

**Job Corps participation led to substantial increases in the receipt of GED and vocational certificates, but it led to slight reductions in the attainment of a high school diploma (Figure 4).** Job Corps had large effects on the receipt of certificates that it emphasizes. Among those without a high school credential at random assignment, about 42 percent of program group members (and 46 percent of program group participants) obtained a GED during the 48-month period, compared to only 27 percent of control group members (an impact of 15 percentage points per eligible applicant). Similarly, more than 37 percent of program group members (and 45 percent of Job Corps participants) reported receiving a vocational certificate, compared to about 15 percent of control group members (an impact of 22 percentage points).

Among those without a credential at baseline, a slightly higher percentage of control group members than program group members obtained a high school diploma (7.5 percent, compared to 5.3 percent). As noted above, although many of the younger control group members attended high school, most of those in high school did not complete it, because they attended high school for an average of only about nine months.

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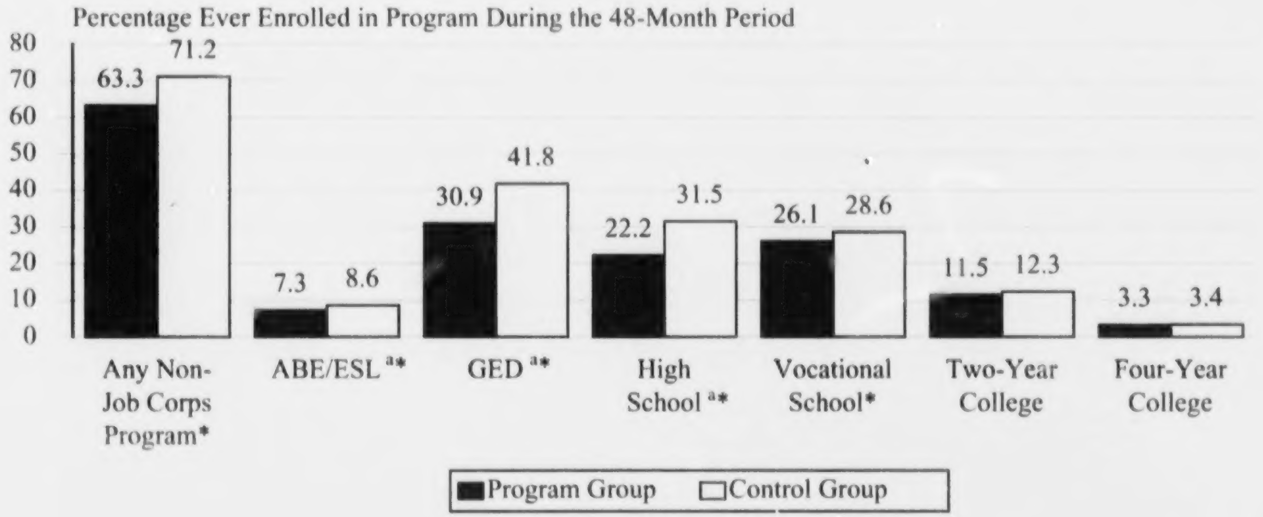
[6] About 15 percent of Job Corps participants attended an education or training program during the follow-up period before they enrolled in Job Corps (that is, between their random assignment and Job Corps enrollment dates). Not surprisingly, most of this activity was high school. About one-half of Job Corps participants enrolled in an education or training program after leaving Job Corps. About 72 percent of the no-shows enrolled in a program during the 48-month period.

## FIGURE 3

## PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM



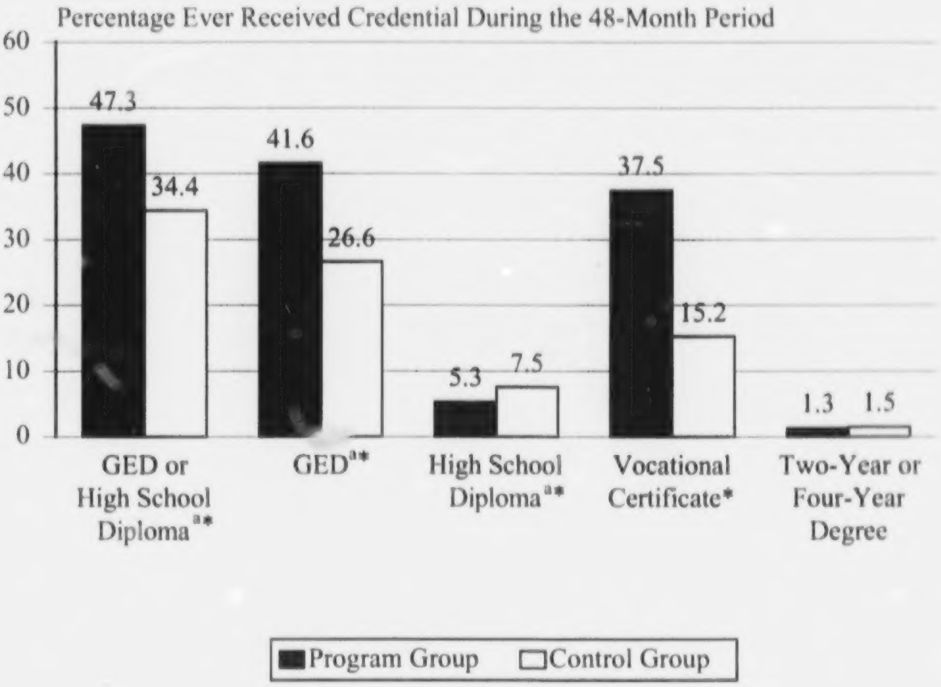
Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a]Figures pertain to those who did not have a high school diploma or GED at random assignment.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

FIGURE 4

DEGREES, DIPLOMAS, AND CERTIFICATES RECEIVED



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a]Figures pertain to those who did not have a high school credential at random assignment.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

**Job Corps had no effect on college attendance and completion (Figures 3 and 4).** About 12 percent of each research group attended a two-year college, and about 3 percent attended a four-year college. Less than 2 percent obtained a two- or four-year college degree.

**Impacts on education and training were large across all subgroups defined by youth characteristics.** Impacts on total time spent in programs and on the attainment of a GED (among those without a high school credential at baseline) or a vocational certificate were very large and statistically significant for all key subgroups. However, the pattern of impacts across subgroups defined by age at application to Job Corps exhibited some differences. There were no impacts on hours in academic classes for those 16 and 17, because nearly half of all control group members who were 16 and 17 attended academic classes in high school. However, large impacts were found on hours spent in academic classes for the older youth, and on hours spent in vocational training for all age groups.

Of particular note, impacts were similar for those assigned to the residential and nonresidential components. This is consistent with findings from the process analysis (Johnson et al. 1999) that nonresidential students are fully integrated into the academic and vocational components of Job Corps.

## EMPLOYMENT AND EARNINGS

We have seen that Job Corps participation leads to large impacts on time spent in academic classes and vocational training and on the attainment of GED and vocational certificates. These large impacts could increase participants' skill levels and, hence, their labor market productivity. This increased productivity may in turn enhance the time spent employed, earnings, wage rates, and fringe benefits of participants after they leave the program.

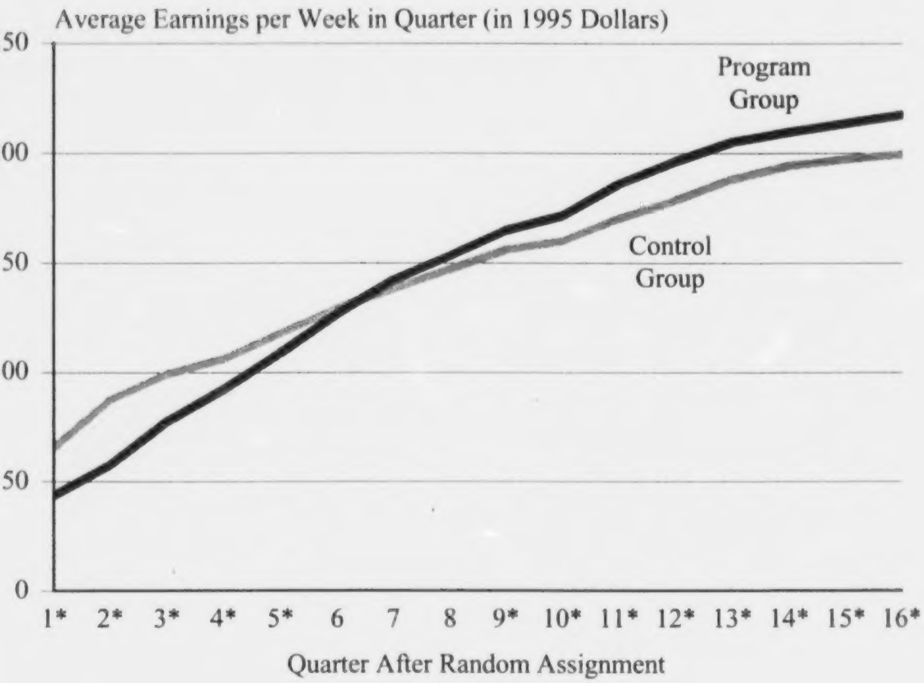
We expect negative impacts on participants' employment and earnings during the period of enrollment, because some would have held jobs if they had not gone to Job Corps. However, because of improvements in participants' skills, we expect positive impacts on employment and earnings after they leave the program and after a period of readjustment. In light of the variation in the duration of program participation and the period of readjustment, it is difficult to predict when positive impacts will emerge.

A summary of our findings is as follows:

**Job Corps generated positive earnings impacts beginning in the third year after random assignment, and the impacts persisted through the end of the 48-month follow-up period (Figure 5 and Table 3).** As expected, the earnings of the control group were larger than those of the program group early in the follow-up period, because many program group members were enrolled in Job Corps then. It took about two years from random assignment for the earnings of the program group to overtake those of the control group. The impacts grew between quarters 8 and 12 (that is, in year 3), and remained fairly constant from quarters 13 to 16 (that is, they *persisted* in year 4). In year 4, average weekly earnings for program group members were $16 higher than for control group members ($211, compared to $195). The estimated year 4 impact per Job Corps *participant*

FIGURE 5

AVERAGE EARNINGS PER WEEK, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE 3

## IMPACTS ON EARNINGS, EMPLOYMENT RATES, AND TIME EMPLOYED IN QUARTERS 13 TO 16 (YEAR 4)

| | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Estimated Impact per Participant[b] |
|---|---|---|---|---|
| Average Earnings per Week, by Quarter After Random Assignment | | | | |
| 13 | 205.3 | 188.0 | 17.3* | 24.1* |
| 14 | 209.8 | 194.2 | 15.7* | 21.8* |
| 15 | 213.7 | 197.2 | 16.5* | 22.9* |
| 16 | 217.5 | 199.4 | 18.1* | 25.2* |
| Percentage Employed, by Quarter | | | | |
| 13 | 66.8 | 63.4 | 3.4* | 4.8* |
| 14 | 67.5 | 65.1 | 2.4* | 3.3* |
| 15 | 69.2 | 65.6 | 3.6* | 5.0* |
| 16 | 71.1 | 68.7 | 2.4* | 3.3* |
| Average Percentage of Weeks Employed, by Quarter | | | | |
| 13 | 58.6 | 55.7 | 3.0* | 4.1* |
| 14 | 59.6 | 56.8 | 2.9* | 4.0* |
| 15 | 60.9 | 57.7 | 3.2* | 4.4* |
| 16 | 61.8 | 59.0 | 2.8* | 3.9* |
| Average Hours Employed per Week, by Quarter | | | | |
| 13 | 26.8 | 25.4 | 1.5* | 2.0* |
| 14 | 27.3 | 25.9 | 1.4* | 1.9* |
| 15 | 27.7 | 26.3 | 1.5* | 2.0* |
| 16 | 27.9 | 26.4 | 1.5* | 2.0* |
| **Sample Size** | **6,828** | **4,485** | **11,313** | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period.

*Significantly different from zero at the .05 level, two-tailed test.

was $22 per week (or $1,150 in total), which translates into a 12 percent earnings gain. These year 4 impacts are statistically significant at the 1 percent significance level.

Over the whole period, Job Corps participants earned about $3 per week (or $624 overall) more than they would have if they had not enrolled in Job Corps. This impact, however, is not statistically significant.

**Job Corps also had statistically significant impacts on the employment rate and time spent employed beginning in year 3 (Figure 6 and Table 3).** The impacts on the employment-related measures were negative during the in-program period. They became positive in quarter 8, increased sharply between quarters 8 and 12, and remained fairly constant afterwards. In year 4, the average quarterly impact on the employment rate was about 3 percentage points per eligible applicant (69 percent for the program group, compared to 66 percent for the control group). The year 4 impact on hours employed per week was 1.4 hours per eligible applicant (27.4 hours for the program group, compared to 26 hours for the control group).

**The earnings gains late in the period were due to a combination of greater hours of work and higher earnings per hour.** Program group members earned about $11 more per week in year 4 than control group members because they worked more hours, and they earned about $5 more per week because they had higher earnings per hour. These gains sum to the $16 impact on earnings per week in year 4.

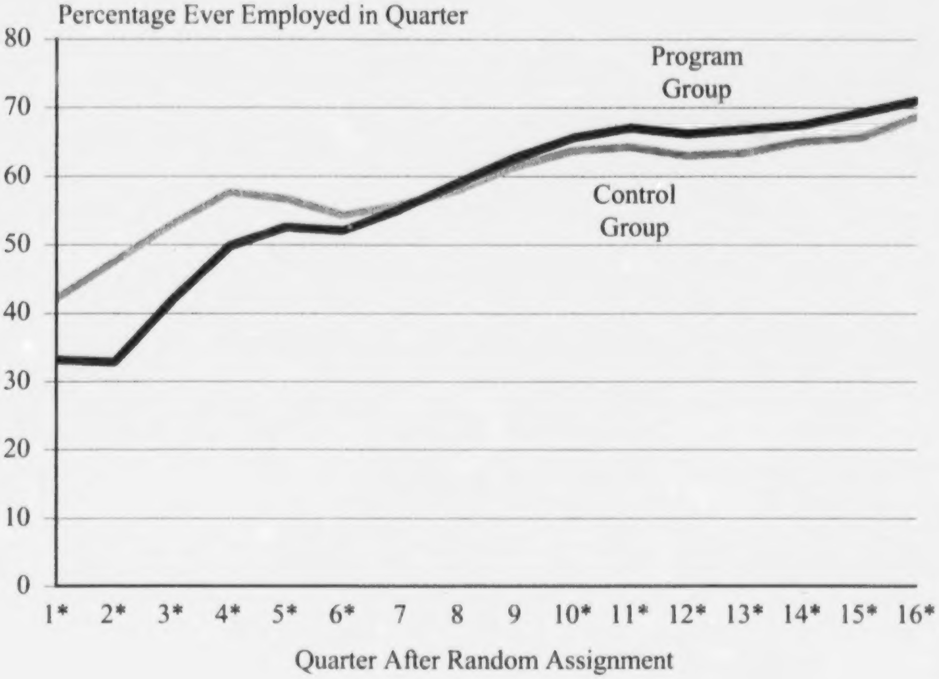
**Program group members secured higher-paying jobs with slightly more benefits in their most recent jobs in quarters 10 and 16.** These findings are consistent with our findings from the literacy study (Glazerman et al. 2000) that Job Corps increases participants' skill levels and, hence, productivity. Employed program group members earned an average of $0.24 more per hour than employed control group members in their most recent job in quarter 10 ($6.77, compared to $6.53), and an average of $0.22 more per hour in their most recent job in quarter 16 ($7.55, compared to $7.33). Furthermore, the wage gains were similar across broad occupational categories, although similar percentages of program and control group members worked in each occupational area in both quarters.

Employed program group members were slightly more likely to hold jobs that offered fringe benefits in quarters 10 and 16. For example, in quarter 16, about 57 percent of the employed program group received health insurance, compared to 54 percent of the employed control group (a statistically significant increase of 3 percentage points, or nearly 6 percent). Similarly, about 48 percent of employed program group members were offered retirement or pension benefits, compared to 44 percent of employed control group members.

**Earnings gains were found broadly across most key subgroups defined by youth characteristics at random assignment.** Earnings gains during the postprogram period were very similar for males and females. Positive earnings impacts were found for groups of students at special risk of poor outcomes (such as very young students, females with children, youths who had been arrested for nonserious offenses, and older youths who did not possess a high school credential at baseline), *as well as* for groups at lower risk (such as older students with a high school credential at baseline). Impacts were similar for youth who applied to the program before or after the new ZT

FIGURE 6

EMPLOYMENT RATES, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

policies took effect, and for whites and African Americans.

**Job Corps did not increase the employment and earnings of Hispanic youths and 18- and 19-year-olds.** We are not able to provide a satisfactory explanation for these findings, although we have been able to rule out several possibilities. In particular, the lack of an impact is not due to differences in Job Corps enrollment rates or length of time in the program. Hispanics had similar enrollment rates as non-Hispanics, and Hispanic students participated for more than a month *longer,* on average than non-Hispanics. Job Corps participation measures did not differ by age.

The lack of impacts also does not appear to be related to other personal or family characteristics associated with low impacts. Overall, the characteristics of Hispanic students and African American participants are very similar (apart from primary language and region of residence), and the characteristics of those 18 and 19 are not unusual. We also found smaller impacts for Hispanic than non-Hispanic students and for those 18 and 19 compared with those in other age groups across nearly all subgroups defined by other key youth characteristics.

Language barriers do not explain the Hispanic findings, as we found similar impacts for Hispanic students whose primary language was English and for those whose primary language was Spanish. Finally, the findings are not due to characteristics of centers or regions in which Hispanic or 18- and 19-year-old students are concentrated. The patterns of impacts by race and ethnicity were similar for sample members designated for centers with a high concentration of Hispanic students and for those designated for centers with a lower concentration.[7] Similarly, impacts were smaller for Hispanic than non-Hispanic students both in regions with a high concentration of Hispanics and in other regions. Centers attended by those 18 and 19 were similar to centers attended by older participants.

**The residential program component was effective for broad groups of students it served.** Earnings and employment impacts in years 3 and 4 for those assigned to the residential component were positive overall, and they were similar for residential males, females with children, and females without children.

**The nonresidential component was also effective for the students it served.** Participation in the nonresidential component improved postprogram earnings overall. It improved average earnings per week in year 4 by more than $35 for females with children (an increase of 24 percent), and by more than $55 for males (an increase of 26 percent). The nonresidential component had no effect, however, on females without children.

We emphasize again that the impact findings by residential status should be interpreted with caution. As discussed, our estimates provide information about the effectiveness of each component for the populations it serves. The estimates cannot be used to assess how a youth in one component would fare in the other one, or how effective each component would be for the average Job Corps student. This is because the characteristics of residents differ from those of nonresidents in ways that can affect outcomes.

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[7] These impacts were estimated using information provided by OA counselors on the center to which each eligible applicant in our study population was likely to be assigned. This information was collected prior to random assignment, and thus is available for both program and control group members.

## WELFARE, CRIME, ILLEGAL DRUG USE, AND OTHER OUTCOMES

The study examined the impacts of Job Corps on several additional outcomes to help assess whether the program achieves its goals of helping students become more responsible and productive citizens. This section reports on impacts on welfare dependence; involvement with the criminal justice system; use of tobacco, alcohol, and illegal drugs; the overall health of participants; the likelihood of bearing or fathering children while unmarried; custodial responsibility; the likelihood of forming stable, long-term relationships; mobility; and the use of child care.

Our main results are as follows:

**Job Corps participation reduced the receipt of public assistance benefits (Table 4).** Overall, program group members reported receiving about $460 less in benefits (across several public assistance programs) than control group members, and this impact is statistically significant at the 1 percent level. The estimated average reduction per participant was $640. The estimated program impacts on the receipt of individual types of assistance were small and in many cases not statistically significant. The number of months receiving AFDC/TANF benefits differed by just 0.4 months (5.0 months for the program group and 5.4 months for the control group). Control group members received food stamps for slightly more months on average than program group members (7.0 months, compared to 6.5 months). Impacts on the receipt of GA, SSI, and WIC benefits and on the likelihood of being covered by public health insurance were small.

Contrary to our expectations that reductions in welfare benefits would be concentrated during the in-program period, when students' material needs were met by the program, the reductions in benefit receipt were fairly uniform across the 48-month follow-up period. To some extent, this reflects different time patterns of the impacts for different groups. The benefit reductions for males were uniform throughout the follow-up period. For females without children at baseline, benefit reductions were largest early in the follow-up period and then declined to nearly zero. In contrast, the benefit reductions for females with children at baseline, many of whom were nonresidential students, were negligible during the in-program period, when welfare helped support the participant and her child, but became larger during the postprogram period, when earnings also increased.

**Job Corps participation significantly reduced arrest and conviction rates, as well as time spent in jail (Table 4).** About 33 percent of control group members were arrested during the 48-month follow-up period, compared to 29 percent of program group members (a statistically significant impact of -4 percentage points per eligible applicant). The impact per participant was about -5 percentage points, which translates to a 16 percent reduction in the arrest rate. Arrest rate reductions were largest during the first year after random assignment (when most program enrollees were in Job Corps). Interestingly, however, Job Corps also led to small arrest reductions during the later months of the follow-up period, after most youths had left Job Corps.

## TABLE 4

## IMPACTS ON KEY PUBLIC ASSISTANCE AND CRIME OUTCOMES

| | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Estimated Impact per Participant[b] |
|---|---|---|---|---|
| Average Amount of Benefits Received, by Year (in Dollars) | | | | |
| All years | 3,696.0 | 4,155.7 | -459.8* | -638.9* |
| 1 | 1,109.8 | 1,225.9 | -116.2* | -161.4* |
| 2 | 978.7 | 1,101.6 | -122.9* | -170.8* |
| 3 | 893.3 | 1,001.4 | -108.1* | -150.2* |
| 4 | 745.5 | 825.6 | -80.1* | -111.3* |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | |
| All years | 28.8 | 32.6 | -3.7* | -5.2* |
| 1 | 11.1 | 14.1 | -3.1* | -4.3* |
| 2 | 10.5 | 11.3 | -0.8 | -1.2 |
| 3 | 11.1 | 11.4 | -0.4 | -0.5 |
| 4 | 9.6 | 10.3 | -0.7 | -0.9 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment | 22.1 | 25.2 | -3.1* | -4.3* |
| Percentage Served Time in Jail for Convictions During the 48-Month Period | 15.8 | 17.9 | -2.1* | -2.9* |
| Average Weeks in Jail for Convictions During the 48-Month Period | 6.0 | 6.6 | -0.6 | -0.8 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period.

[c] Benefits include AFDC/TANF, food stamps, SSI/SSA, and General Assistance.

*Significantly different from zero at the .05 level, two-tailed test.

Program group members were less likely to have arrest charges for nearly all categories of crimes. However, reductions were slightly larger for less serious crimes (such as disorderly conduct and trespassing).

Job Corps participation also reduced convictions and incarcerations resulting from a conviction. More than 25 percent of control group members were ever convicted during the follow-up period, compared to 22 percent of program group members. Similarly, Job Corps reduced the percentage incarcerated for convictions by 2 percentage points (from 18 percent to 16 percent) and the average time spent in jail by about six days.

Although the level of criminal activity differed substantially across youth subgroups, the impacts on crime outcomes were very similar (in particular, by gender and age). We find some differences, however, in crime impacts by residential status. Job Corps reduced arrest rates for male residents, female residents, and female nonresidents. However, the program had no effect for male nonresidents.

**Job Corps participation led to reductions in crimes committed against program participants.** On average, Job Corps reduced the average number of victimizations by about 130 victimizations per thousand during the first 12 months after random assignment--a 20 percent reduction. As expected, the frequency of victimizations was reduced most during the in-program period, but the reductions persisted somewhat afterwards. Reductions were found for almost every crime type, and across most subgroups.

**Job Corps had no impacts on the self-reported use of tobacco, alcohol, and illegal drugs.** This finding applied for the full sample and for key subgroups. Job Corps also had little effect on time spent in drug treatment.

**Job Corps improved participants' perceived health status.** At each interview, about 17.5 percent of the control group and 15.5 percent of the program group said their health was "poor" or "fair."

**Job Corps had no impacts on fertility or custodial responsibility, either for the full sample or by gender.** About 38 percent of those in both the program and control groups had a child during the follow-up period (49 percent of females and 31 percent of males), and more than 80 percent of children were born out of wedlock. About two-thirds of all parents (and 42 percent of male parents) were living with all their children, and about 82 percent of male parents provided support for noncustodial children.

**Job Corps participation slightly promoted independent living at the 48-month interview point.** A slightly smaller percentage of program group members were living with their parents (32 percent, compared to 35 percent of control group members), and a slightly larger percentage were living with a partner either married or unmarried (31 percent, compared to 29 percent). Furthermore, program group members were more likely to report being the head of their household (52 percent, compared to 50 percent). This same pattern holds for males and females with and without children at baseline.

**Job Corps slightly increased mobility, but had no impact on the types of areas in which participants lived at the 48-month interview point.** Program group members were slightly less likely than control group members to have lived less than 10 miles from where they lived at application (73 percent, compared to 75 percent of the control group), and were slightly more likely to have lived more than 50 miles away (17 percent, compared to 16 percent). Thus, the average distance between the zip codes of residence at application to Job Corps and at the 48-month interview was slightly larger for the program group (94 miles, compared to 86 miles). The average characteristics of the counties of residence at 48 months, however, were similar for program and control group members. Furthermore, they were similar to the average county characteristics of residence at the time the youths applied to Job Corps (because most youths lived in the same areas at program application and at 48 months).

**Job Corps participation led to increases in the use of child care.** During the 48-month period, Job Corps participants used an average of about 146 more hours of child care than they would have if they had not enrolled in Job Corps.[8] Impacts on child care use were positive during the first year after random assignment (when many program group members were enrolled in Job Corps) and during the fourth year (when employment impacts were the largest), but not in years 2 and 3. Impacts were found for females but not for males, because only a small percentage of fathers were living with their children and needed to find child care.

## CONCLUDING OBSERVATIONS

**Job Corps provided participants with the instructional equivalent of one additional year in school.** Enrollees reported receiving extensive Job Corps services. Overall, they received an average of about 1,000 hours of education and training that they would not have received otherwise. This is approximately the hours of instruction delivered in a typical school year. These impacts on education and training could have led to the postprogram earnings gains we observed.

Of course, Job Corps also provides other services that could have contributed to the postprogram earnings gains. It provides a residential living program, health care, and a broad range of services designed to help youth who have not succeeded in school to become productive young adults. Many staff and observers of the program believe that the distinctive residential component of Job Corps is a key ingredient, both because the residential component is necessary for delivering effective academic and vocational instruction and because the experience of living in a community committed to learning has intrinsic benefits apart from the formal education and training that Job Corps provides. Because of the comprehensive nature of Job Corps, it is difficult to determine the relative contributions of the different parts of the program to the beneficial impacts that we find. However, viewing Job Corps as providing an additional year of schooling offers a way to place the earnings impacts into perspective.

**Earnings gains observed beginning in the third year after random assignment are commensurate with what would be expected from an additional year of school.** Economists have long been concerned about the returns to schooling. They pose the question, How much difference does an additional year of schooling make in the lifetime earnings of an individual? The answers they have developed over the last two decades provide an important perspective on the study's findings.

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[8] Child care use pertains only to arrangements used by parents while they were working or attending education and training programs.

Studies of the average returns to a year of schooling consistently find that a year of schooling increases earnings over a worker's lifetime by 8 to 12 percent. Measured in hours spent in academic classes and vocational training, Job Corps provided roughly the equivalent of a year of additional schooling per participant. In this context, the 12 percent earnings gains and the persistence of the earnings gains during the latter part of the 48-month period are in line with what one would expect from an intensive education and training program that serves primarily school-aged youth.

**Most subgroups of students benefited from Job Corps.** The finding that Job Corps improves key outcomes for broad groups of students rather than for only a subset provides further evidence that the program is effective. Participation led to substantial improvements in education-related outcomes for all subgroups of students that we investigated. Employment and earnings gains were similar for males and females. Postprogram earnings gains were found for groups of students at special risk of poor outcomes (such as very young students, females with children, those arrested for nonserious crimes, and older youths who did not possess a high school credential at baseline), *as well as* for groups at lower risk (such as older students with a high school credential at baseline). The program increased earnings for whites as well as for African Americans (although earnings gains were not found for Hispanics), and for those who applied before and after the ZT policies took effect. Reductions in criminal activity were found for nearly all groups of students. Thus, Job Corps effectively serves a broad group of students with differing abilities and needs.

While Job Corps is broadly effective, the impacts for several particularly vulnerable or difficult-to-serve groups are especially noteworthy.

**Beneficial program impacts were found for 16- and 17-year-old youth.** For this group: (1) average earnings gains per participant were nearly $900 in year 4, (2) the percentage earning a high school diploma or GED was up by 66 percent, and (3) arrest rates were reduced by 11 percent and rates of incarceration for a conviction by 19 percent. While staff find this group difficult to deal with, and while more of them leave Job Corps before completing their education and training than do older students, the youngest age group does appear to benefit from their program experiences.

**Females with children at the time of enrollment enjoyed significant earnings gains and modest reductions in welfare receipt.** More than one-half of young women with children enrolled in Job Corps as nonresidential students, because child-rearing responsibilities required that they live at home. However, these young women received similar amounts of academic classroom instruction and vocational training as other students, despite living at home. Furthermore, in year 4, they enjoyed increases of more than 20 percent in their earnings and reductions of about 12 percent in their receipt of public assistance.

**The residential and nonresidential programs serve different groups of students, and each is effective for the groups it serves.** Earnings and employment impacts during the last two years of the follow-up period were positive overall for those assigned to each component. Furthermore, earnings gains were positive in each component for nearly all subgroups defined by gender and the presence of children at random assignment.

Importantly, it is *not* appropriate to conclude that the residential component could be abolished and everyone served just as well in the less expensive nonresidential component, for several reasons. First, the two components serve very different students. Nonresidential students tend to be females with children and older youths who would be unable to participate in the residential Job Corps program because of family responsibilities. On the other hand, residential students tend to be younger and less educated, and are deemed by Job Corps staff to require training in a residential setting to fully benefit from the program. Consequently, our results cannot be used to assess how students in the residential component (for example, 16- and 17-year-old residents) would fare in the nonresidential component.

Second, most centers with nonresidential slots also have residential slots, so nearly all nonresidential students train with residential students and may benefit from interacting with them. The program experiences of nonresidential students would probably be much different if the residential component were abolished.

Finally, nonresidential students receive services that are similar in many ways to those received by residential students, and the nonresidential component of Job Corps is more intensive and comprehensive than most other nonresidential training programs. In fact, the program cost per nonresidential student is only about 16 percent less than the program cost per residential student (McConnell et al. 2001). Thus, the cost of Job Corps would not be reduced significantly if all students were served in the nonresidential component.

In conclusion, we find that Job Corps produces beneficial impacts on the main outcomes that it intends to influence. Beneficial impacts on education-related, employment-related, and crime-related outcomes were found overall, as well as for broad subgroups of students. The residential and nonresidential program components were each effective for the students they served. A companion report, presenting findings from the benefit-cost analysis, concludes that Job Corps is a worthwhile investment both for the students and for the broader society that supports their efforts.

# I. INTRODUCTION

Job Corps plays a central role in federal efforts to provide employment assistance to disadvantaged youths ages 16 to 24. The program's goal is to help disadvantaged youths become "more responsible, employable, and productive citizens" by providing comprehensive services, including basic education, vocational skills training, counseling, and residential support. Each year, Job Corps serves more than 60,000 new enrollees and costs more than $1 billion.

The National Job Corps Study, funded by the U.S. Department of Labor (DOL), was designed to provide information about the effectiveness of Job Corps in attaining it goal.[1] The cornerstone of the study was the random assignment of all youths found eligible for Job Corps to either a program group or a control group. Program group members were permitted to enroll in Job Corps, and control group members were not (although they could enroll in other training or education programs). The research sample for the study consists of approximately 9,400 program group members and 6,000 control group members randomly selected from among nearly 81,000 eligible applicants nationwide. Sample intake occurred between November 1994 and February 1996.

This report presents estimates of the impacts of Job Corps on participants' employment and related outcomes during the 48 months after random assignment. The report addresses the following research questions:

- How effective is Job Corps overall at improving the employability of disadvantaged participants?

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[1] The study is being conducted by Mathematica Policy Research, Inc. (MPR) and its subcontractors, Battelle Human Affairs Research Centers and Decision Information Resources, Inc.

- Do Job Corps impacts differ for youths with different characteristics?
- How effective are the residential and nonresidential components of Job Corps?

To examine these questions, we estimated the impact of Job Corps on key outcome measures by comparing the distribution of outcomes of program and control group members, for the full sample and for key subgroups. The outcome measures for the analysis were constructed using follow-up survey data collected 12, 30, and 48 months after random assignment, and key subgroups were defined using baseline interview and program intake data. The findings presented here update those presented in our report on the short-term program impacts over the first two and a half years after random assignment (Schochet et al. 2000).

The rest of the report begins in Chapter II with an overview of the Job Corps program and the National Job Corps Study (with a focus on the design of the impact study). Chapter III describes data sources, outcome measures, and analytic methods used for the analysis. Chapter IV provides a brief summary of the Job Corps experiences of those in the program group. These three chapters provide important background and contextual information to aid in the interpretation of study findings. Chapters V, VI, and VII present impact estimates on the following categories of outcome measures that we hypothesized could be influenced by participation in Job Corps: (1) education and training; (2) employment, earnings, and job characteristics; and (3) nonlabor market outcomes, including the receipt of public assistance and other sources of income; criminal activities; tobacco, alcohol, and illegal drug use; and health, family formation, child care, and mobility.

## II. OVERVIEW OF JOB CORPS AND THE NATIONAL JOB CORPS STUDY

Job Corps is an intensive and comprehensive program whose goal is to help disadvantaged youths become "more responsible, employable, and productive citizens." The first part of this chapter summarizes the operational structure of Job Corps, key program elements, and the characteristics of youths who apply for the program and are determined to be eligible. The second part of the chapter provides an overview of the National Job Corps Study, including the primary research questions and the main study features that are being employed to assess the effectiveness of Job Corps. The focus of this section is to describe the study design for the impact analysis.

### A. OVERVIEW OF JOB CORPS

The Job Corps program, established by the Economic Opportunity Act of 1964, operates under provisions of the Workforce Investment Act (WIA) of 1998.[1] The operational structure of Job Corps is complex, with multiple levels of administrative accountability, several distinct program components, and numerous contractors and subcontractors. DOL administers Job Corps through a national office and nine regional offices. The national office establishes policy and requirements, develops curricula, and oversees major program initiatives. The regional offices procure and administer contracts and perform oversight activities, such as reviews of center performance.

Through its regional offices, DOL uses a competitive bidding process to contract out center operations, recruiting and screening of new students, and placement of students into jobs and other educational opportunities after they leave the program. At the time of the study, 80 centers were operated under such contracts. In addition, the U.S. Departments of Agriculture and of the Interior operated 30 centers, called Civilian Conservation Centers (CCCs), under interagency agreements with DOL. Figure II.1 shows the location of the 105 Job Corps centers in the contiguous 48 states and the District of Columbia that were in operation at the time our program group members were enrolled, and displays the nine Job Corps regions.[2,3]

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[1] For much of the study, Job Corps operated under provisions of the Job Training Partnership Act (JTPA) of 1982.

Next, we briefly outline the roles of the three main program elements and then highlight key characteristics of youths served by the program. The section concludes with a discussion of major policy changes that occurred during the study period. The process analysis report for the evaluation provides more details on these topics (Johnson et al. 1999).

### 1. Outreach and Admissions

Outreach and admissions (OA) agencies conduct recruitment and screening for Job Corps. OA agencies include private nonprofit firms, private for-profit firms, state employment agencies, and the centers themselves. These agencies provide information to the public through outreach activities (for example, by placing advertisements and making presentations at schools), screen youths to ensure that they meet the eligibility criteria, assign youths to centers (when the regional office delegates this function), and arrange for transportation to centers.

### 2. Job Corps Center Services

Job Corps is a comprehensive and intensive program. Its major components include basic education, vocational training, residential living (including training in social skills), health care and education, counseling, and job placement assistance. Services in each of these components are tailored to each participant.

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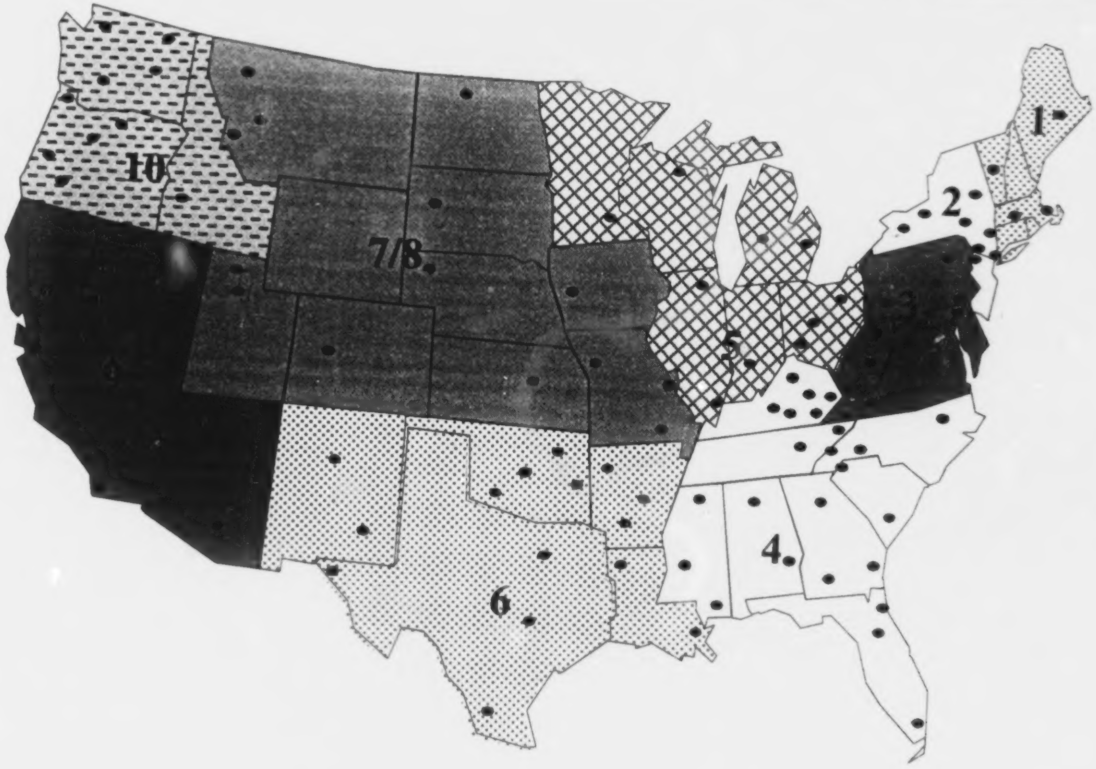
[2] In total, there were 110 centers in operation, including the five centers in Alaska, Hawaii, and Puerto Rico.

[3] There are currently 119 centers in operation.

# FIGURE II.1

# JOB CORPS CENTERS IN PROGRAM YEAR 1995, BY REGION



• Indicates one of the 105 Job Corps Centers in the contiguous 48 States and the District of Columbia.

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**Education.** The goal of the education component is to enable students to learn as fast as their individual abilities permit. Education programs in Job Corps are individualized and self-paced, and they operate on an open-entry and open-exit basis. The programs include remedial education (emphasizing reading and mathematics), world of work (including consumer education), driver education, home and family living, health education, programs designed for those whose primary language is not English, and a General Educational Development (GED) program of high school equivalency for academically qualified students. About one-fourth of the centers can grant state-recognized high school diplomas.

**Vocational Training.** The vocational training programs at Job Corps, like the education component, are individualized and self-paced and operate on an open-entry and open-exit basis. Each Job Corps center offers training in several vocations, typically including business and clerical, health, construction, culinary arts, and building and apartment maintenance. National labor and business organizations provide vocational training at many centers through contracts with the Job Corps national office.

**Residential Living.** Residential living is the component that distinguishes Job Corps from other publicly funded employment and training programs. The idea behind residential living is that, because most participants come from disadvantaged environments, they require new, more supportive surroundings to derive the maximum benefits from education and vocational training. All students must participate in formal social skills training. The residential living component also includes meals, dormitory life, entertainment, sports and recreation, center government, center maintenance, and other related activities. Historically, regulations had limited the number of nonresidential students to 10 percent, but Congress raised that limit to 20 percent in 1993.

**Health Care and Education.** Job Corps centers offer comprehensive health services to both residential and nonresidential students. Services include medical examinations and treatment; biochemical tests for drug use, sexually transmitted diseases, and pregnancy; immunizations; dental examinations and treatment; counseling for emotional and other mental health problems; and instruction in basic hygiene, preventive medicine, and self-care.

**Counseling and Other Ancillary Services.** Job Corps centers provide counselors and residential advisers. These staff help students plan their educational and vocational curricula, offer motivation, and create a supportive environment. Support services are also provided during recruitment, placement, and the transition to regular life and jobs following participation in Job Corps.

### 3. Placement

The final step in the Job Corps program is placement, which helps students find jobs in training-related occupations with prospects for long-term employment and advancement. Placement contractors may be state employment offices or private contractors, and sometimes the centers themselves perform placement activities. Placement agencies help students find jobs by providing assistance with interviewing and resume writing and services for job development and referral. They are also responsible for distributing the readjustment allowance, a stipend students receive after leaving Job Corps.

### 4. Characteristics of Youths Served by Job Corps

To participate in Job Corps, youths must be legal U.S. residents ages 16 to 24. Males 18 or older must be registered with the Selective Service Board, and minors must have the consent of a parent or guardian. Youths must also be disadvantaged (defined as living in a household that receives welfare or has income below the poverty level) and living in a debilitating environment that substantially impairs prospects for participating in other programs. Youths must need additional education, training, and job skills and possess the capacity and aspirations to benefit from Job Corps. They must also be free of serious behavioral and medical problems, and they must have arranged for adequate child care (if necessary) when they participate in Job Corps.

The detailed information from the study's baseline interview provides insights about the backgrounds of eligible Job Corps applicants (Schochet 1998a). Most eligible applicants are male (60 percent), and most are younger than 20 (40 percent are 16 or 17 years old, and nearly one-third are 18 or 19). About 40 percent live in the South, and more than 70 percent are members of racial or ethnic minority groups: 50 percent are African American, 18 percent are Hispanic, 4 percent are Native American, and 2 percent are Asian or Pacific Islander. Most (nearly 80 percent) do not have a high school credential. About 18 percent have children, and nearly 60 percent received some form of public assistance during the year prior to random assignment. About one-quarter reported that they had ever been arrested, and about 30 percent reported using illegal drugs in the year prior to random assignment.

The characteristics of eligible applicants differ by gender and age. Female applicants tend to be older than male applicants, and a higher percentage have children (29 percent, compared to 11 percent). Consequently, a much higher percentage of females (and especially females with children) are assigned to the nonresidential component. Females are more likely to have a high school credential (27 percent, compared to 17 percent of males) at the time of program application, in part because they are older. Females are also less likely to report having used illegal drugs in the prior year (25 percent, compared to 35 percent of males) or ever having been arrested (17 percent, compared to 33 percent of males).

Many of the differences across age groups would be expected. For example, older applicants are much more likely than younger applicants to have been recently employed and to have a high school credential (50 percent of those ages 20 to 24 have a credential) and are much less likely to have recently participated in an education program.

Younger eligible applicants exhibit several characteristics that suggest they may be more disadvantaged and harder to serve than older applicants. A higher proportion of younger applicants report having used drugs, having ever been arrested, and having recently been arrested. Furthermore, younger applicants are more likely to come from single-parent households and from families that received public assistance in the prior year.

### 5. Policy Changes Related to Violence and Drugs

In response to congressional concerns about the operation of the Job Corps program, new zero-tolerance (ZT) policies for violence and drugs were instituted in March 1995--early in the sample intake period for the National Job Corps Study. The new policies were instituted to ensure full and consistent implementation of existing policies for violence and drugs. According to the new, stricter ZT policy, students accused of specific acts of violence (possession of a weapon, assault, sexual assault, robbery, extortion, or arson) or arrested for a felony are to be removed from the center immediately and terminated from the program if fact-finding establishes they committed the alleged offenses. The ZT policy for drugs uses the same procedures for students accused of possession or sale of illegal drugs or alcohol on center or convicted of a drug offense.

The policies were intended to facilitate the rapid removal of offending students and to eliminate any discretion of staff regarding termination. Most Job Corps staff reported that the new policies substantially improved the quality of life on centers (Johnson et al. 1999). Thus, the new policies could have affected program impacts. Consequently, as discussed in Chapter III, we computed separate impact estimates for sample members who applied to Job Corps before and after the new ZT policies became effective.

## B. OVERVIEW OF THE NATIONAL JOB CORPS STUDY

The National Job Corps Study addresses six major research questions:

1. How effective is Job Corps overall at improving the employability of disadvantaged youth?
2. Does the effectiveness of Job Corps differ for youths with different personal characteristics or experiences before application to Job Corps? Do impacts vary by gender, age, the presence of children, education level, race and ethnicity, or arrest history?
3. Do program impacts differ for centers with different characteristics? Do impacts vary by CCC or center contractor type, center size, center performance level, or region?
4. Do program impacts differ for enrollees with different program experiences? Do impacts differ by residential status or program completion status?
5. What is the Job Corps program "model," and how well is it implemented in practice?
6. Is Job Corps cost-effective?

The study consists of an impact analysis (to address Questions 1 to 4), a process analysis (to address Question 5), and a benefit-cost analysis (to address Question 6).

This report presents impact estimates for the full sample and for subgroups defined by youth characteristics (to address the first two research questions). This analysis forms the core of the 48-month impact analysis because it provides information about the effectiveness of Job Corps overall and identifies groups of the eligible population that benefit most (and least) from the program. The report also assesses the effectiveness of the residential and nonresidential components. This facet of the overall evaluation is of considerable policy interest for two reasons: (1) the residential component is the distinguishing feature of Job Corps, and (2) previous studies (for example, the

JTPA and JOBSTART evaluations) indicate that disadvantaged youths do not benefit significantly from participation in training programs that offer basic education and job-training services in a nonresidential setting.

Separate reports present impacts for subgroups defined by key center characteristics (to address Question 3; Burghardt et al. 2001) and program completion status (to address the rest of Question 4; Gritz et al. 2001). The purpose of these analyses is to identify program features and components that are particularly effective, so that policymakers can improve program operations and direct future program expansions.

In the rest of this section, we first provide an overview of the sample design for the impact analysis. Second, we review the evidence that the random assignment design was successfully implemented, which would suggest that program impacts can be effectively estimated. More details on these topics are provided in the report on study implementation (Burghardt et al. 1999). Finally, we briefly discuss key features of the process and benefit-cost analyses.

### 1. Impact Analysis

The central feature of the study design was the random assignment of all youths found eligible for Job Corps, either to a program group whose members were permitted to enroll in Job Corps or to a control group whose members were not. DOL considered both random assignment and nonexperimental design options in the initial design stages of the study. Because of the need for reliable, credible information about program impacts, a study advisory panel, which included representatives of Job Corps, concluded that a random assignment design was feasible and should be used for the study.

### a. Sample Design

Sample intake occurred between November 1994 and February 1996. With few exceptions, all youths who applied to Job Corps for the first time between November 16, 1994, and December 17, 1995, and were found eligible for the program were included in the study--a total of 80,883 eligible applicants. During the sample intake period, 5,977 Job Corps-eligible applicants were randomly selected to the control group. Approximately 1 eligible applicant in 14 (seven percent of 80,883 eligible applicants) was assigned to the control group.

During the same 16-month period, 9,409 eligible applicants were randomly assigned to the research sample as members of the program research group (hereafter referred to as the program group).[4] Because random assignment occurred after youths were determined eligible for Job Corps (and *not* after they enrolled in Job Corps centers), the program group includes youths who enrolled in Job Corps (about 73 percent of eligible applicants), as well as those who did not enroll, the so-called "no-shows" (about 27 percent of eligible applicants). Although the study's research interest focuses on enrollees, all youths who were randomly assigned, including those who did not enroll at a center, were included in the analysis to preserve the benefits of the random assignment design.

Control group members were not permitted to enroll in Job Corps for a period of three years, although they were able to enroll in other programs available to them. Thus, the outcomes of the control group represent the outcomes that the program group would have experienced if they had not been given the opportunity to enroll in Job Corps. Because control group members were allowed to enroll in other education and training programs, the comparisons of program and control group outcomes represent the effects of Job Corps *relative to other available programs* that the study population would enroll in if Job Corps were not an option. The impact estimates do not represent the effect of the program relative to no education or training; instead, they represent the incremental effect of Job Corps.

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[4] The remaining 65,497 eligible applicants were randomly assigned to a program nonresearch group. These youths were allowed to enroll in Job Corps but are not in the research sample.

The National Job Corps Study is based on a fully national sample. With a few exceptions, the members of the program and control groups were sampled from *all* OA agencies located in the contiguous 48 states and the District of Columbia, rather than from only some OA agencies in certain areas.[5] This design feature allows us to obtain impact estimates that are more precise than those that could be obtained from a clustered sample of the same size. In addition, the nonclustered design spread the burden of random assignment across all OA agencies and Job Corps centers, which reduced the burden on any one agency or center.

The sampling rates to the control and program groups differed for some population subgroups for both programmatic and research reasons. For example, OA agencies experienced difficulties recruiting females for residential slots, and Job Corps staff were concerned that the presence of the control group would cause these slots to go unfilled. Therefore, sampling rates to the control group were set lower for females in areas from which high concentrations of residential students come. Because of differences in sampling rates across population subgroups, all analyses were conducted using sample weights so that the impact estimates can be generalized to the intended study population: applicants in the 48 contiguous states and the District of Columbia who applied to Job Corps during the 13-month period between November 17, 1994, and December 16, 1995, and who were determined to be eligible for the program.[6]

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[5] Youths who previously participated in Job Corps ("readmits") or who applied for one of seven small, special Job Corps programs were excluded from the study (see Burghardt et al. 1999).

[6] The study population also included only those whose random assignment forms were received by MPR before March 1, 1996. This restriction did not exclude many eligible applicants who applied to the program during the 13-month period, because the time between program application and eligibility determination is typically very short.

### b. Implementation of Random Assignment

As expected, random assignment produced equivalent groups, because the distribution of the characteristics of program and control group members prior to random assignment was similar (Schochet 1998b). However, our ability to draw valid inferences from a random assignment study depends on three conditions: (1) that all members of the study population were subject to random assignment, (2) that control group members did not enroll in the program, and (3) that operations of the program were not materially affected by the study.

To identify center enrollees in the study population who were not randomly assigned and to ensure that control group members did not enroll, we examined weekly extracts from the Job Corps Student Pay, Allotment, and Management Information System (SPAMIS) on all new center enrollees.

Our monitoring indicates that Job Corps staff implemented random assignment procedures well. Less than 0.6 percent of youths in the study population were not randomly assigned. In addition, only 1.4 percent of control group members enrolled in Job Corps before the end of the three-year period during which they were not supposed to enroll.[7] Hence, we believe that the research sample is representative of the youths in the intended study population and that the bias in the impact estimates due to contamination of the control group is very small.

In general, the study did not appear to alter program operations substantially, which suggests that the study is evaluating Job Corps as it would have normally operated in the absence of the study. We found from the process analysis that the effects of the random assignment process on OA counselors' activities and on the composition of students coming to the program appear to have been modest. For example, few OA counselors said they started new outreach activities, spent more time on outreach, or lost referral sources because of the study. In addition, OA counselors do not appear to have provided substantially more assistance in finding alternative training opportunities to the control group than they provided for other applicants who could not enroll in Job Corps.

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[7] An additional 3.2 percent of control group members enrolled in Job Corps after their three-year restriction period ended and before four years after random assignment (see Chapter III).

The study, however, contributed somewhat to the decrease in the number of center slots that were filled (that is, in center on-board strength) in early 1995, because control group members were removed from the pool of potential center enrollees. We estimate, however, that the introduction of the new ZT policies had a much larger effect on the decrease in center on-board strength. Nonetheless, the study could have had some effect on the training experiences of program group members, as centers served fewer students without reducing center staff.

### 2. Process Analysis

The purpose of the process study was to describe the key elements of the Job Corps program model and to document how they were implemented during calender year 1996--roughly the period when study program group members were enrolled in Job Corps centers (Johnson et al. 1999). The process study collected a large amount of information about OA practices, center operations, and placement from (1) a telephone survey of Job Corps OA counselors, (2) a mail survey of all Job Corps centers, and (3) visits to 23 centers.

The analysis found that Job Corps uses a well-developed program model and is successful in implementing it. Job Corps students are receiving substantial, meaningful education and training services. We refer to process analysis findings in this report because they provide important contextual information to help interpret findings from the impact analysis.

### 3. Benefit-Cost Analysis

The primary purpose of the benefit-cost analysis is to assess whether the benefits of Job Corps are commensurate with the substantial public resources invested in it. The most important benefits that are valued are (1) increased output that may result from the additional employment and productivity of program participants; (2) increased output produced by youths while in Job Corps; (3) reduced criminal activity; and (4) reduced use of other services and programs, including welfare and other educational programs. The most important Job Corps costs include program operating costs and the earnings forgone while the youth attended Job Corps.

The results of the benefit-cost analysis are presented in a companion report (McConnell et al. 2001).

# III. DATA SOURCES, OUTCOME MEASURES, AND ANALYTIC METHODS

We conducted the impact analysis using survey data collected during the 48 months after random assignment. We used data on the experiences of sample members during the follow-up period to construct outcome measures so that the analysis could address the following research questions:

- Do participants receive more education and vocational training than they would have if they had not participated in Job Corps? Are they more likely to obtain a high school credential or vocational certificate?
- Does participation in Job Corps increase productivity and, hence, time spent employed and earnings?
- Does participation in Job Corps reduce dependence on welfare and other public transfers?
- Does Job Corps reduce the incidence and severity of crimes committed by program participants, both during and after the program? Does Job Corps reduce crimes committed against participants?
- Are participants less likely to use tobacco, alcohol, and illegal drugs?
- Does Job Corps reduce the likelihood of bearing or fathering children while unmarried and increase the likelihood of forming a stable, long-term relationship?
- Do participants move to areas that offer opportunities different from those in the areas they came from?

To address these questions, we estimated program impacts by comparing the distribution of outcomes of program and control group members. Program impacts were estimated for the full sample and for key subgroups defined by youth characteristics (using baseline interview data) and whether the youth was designated for a residential or nonresidential slot (using program intake data).

## A. DATA SOURCES

We used four main categories of data for the impact analysis presented in this report:

1. ***Follow-Up Interview Data Collected 12, 30, and 48 Months After Random Assignment.*** We used these data, which contain information on the employment-related and other experiences of sample members during the follow-up period, to construct outcome measures for the impact analysis. Each follow-up interview contains information on the experiences of sample members since the previous interview. We used these data to construct longitudinal outcome measures so that we could examine changes in program impacts over time.

2. ***Baseline Interview Data.*** This information was collected soon after random assignment and contains background information on sample members and their experiences prior to the baseline interview. We used these data to create subgroups defined by youth characteristics at random assignment. We also used them to construct outcome measures that pertain to the period between the random assignment and baseline interview dates.

3. ***Data from Job Corps Intake (ETA-652) Forms.*** These are the standard intake forms that OA counselors and program applicants fill out as part of the application process. They contain basic demographic information on applicants. MPR received these forms as part of the random assignment process and data-entered the information into the computer for those in the research sample. Because this information is available for *all* research sample members, we used it in the nonresponse analysis to compare the characteristics of interview respondents and nonrespondents, and to adjust sample weights to account for the possible effects of interview nonresponse on the impact estimates.

4. ***Data from the Supplemental ETA-652 Forms.*** These forms, which were created for the study, were filled out by OA counselors as part of the application process and were sent to MPR as part of the random assignment process. The forms collected information on whether the youth was likely to be assigned to a residential or a nonresidential slot. As described in more detail later in this chapter, we used this information to estimate program impacts for residential and nonresidential students. The forms also collected information on the center to which a youth was likely to be assigned. We used these data in a separate report that presents program impact estimates for subgroups defined by key center attributes (for example, CCC or contract center type, center performance level, center size, and region).

The impact analysis also uses other data. Functional literacy test score data on a random subsample of the research sample were collected in conjunction with the 30-month interview.

Impact results using these data are presented in Glazerman et al. (2000) and are referred to in this report. In addition, we collected official crime records data from North Carolina and Texas covering the 30-month period after random assignment, and compared crime levels and impacts based on these records to those based on the follow-up interview data (Needels et al. 2000). We also refer to these findings in this report. Future reports will present impact results using administrative data on social security earnings on all sample members and Unemployment Insurance (UI) administrative records from 17 randomly selected states.

The rest of this section provides an overview of the survey design, the interview response rates, and the analysis samples. A separate methodological report (Schochet 2001) discusses these topics in more detail.

### 1. Design of the Baseline and Follow-Up Interviews

Baseline interviewing took place between mid-November 1994 and July 1996. We contacted all sample members by telephone soon after they had been subject to random assignment. We used detailed tracking information (contained in program intake forms sent to MPR as part of the random assignment process) to help locate youths. In randomly selected areas, we attempted in-person interviews with sample members not reachable by telephone within 45 days. To contain data collection costs, we subsampled youths for intensive in-person interviewing.

The target sample for the 12-month follow-up interview included (1) all sample members selected for in-person interviews at baseline (whether interviewed or not), and (2) those not eligible for in-person interviews at baseline who completed the baseline interview by telephone within 45 days after random assignment. Thus, youths who resided in areas not selected for in-person interviews and who did not complete a baseline interview by telephone within 45 days were not eligible for 12-month (and subsequent) interviews. At the end of the 12-month interview, we administered an abbreviated baseline interview to those 12-month respondents in the in-person areas who had not completed the full baseline interview.

We attempted a 30-month interview with all sample members who completed either the baseline or the 12-month interview. Youths eligible for a 48-month interview were those who completed any previous interview. However, to reduce data collection costs, we randomly selected for 48-month interviewing about 93 percent of program group members who were eligible for 48-month interviews. We asked respondents to the 30- and 48-month interviews about their experiences since their previous interview.

For the 12-, 30-, and 48-month interviews, we first attempted interviews by telephone and then, if we were unsuccessful, in person. In contrast to the in-person interviewing at baseline, there was no clustering of in-person interviews in the follow-up interviews. We conducted the 12-month interview between March 1996 and September 1997, the 30-month interview between September 1997 and February 1999, and the 48-month interview between December 1998 and May 2000.

We offered a $10 incentive fee to control group members and hard-to-locate program group members (who were not at a Job Corps center) to induce them to complete each interview. In June 1999, however, we increased the incentive fee to $25 to boost the response rate to the 48-month interview.

### 2. Response Rates and Data Quality

The response rate to the baseline interview for sample members in all areas was 93.1 percent. We completed interviews with 14,327 of the 15,386 youths in the research sample, most by telephone soon after random assignment. Furthermore, the difference in completion rates between the program and control groups was only 1.5 percentage points (93.8 percent program, 92.3 control). The response rate for sample members in the areas selected for in-person interviewing--the *effective* response rate--was 95.2 percent (95.9 percent program, 94.3 percent control). Response rates to the baseline interview were high for all key subgroups. Item nonresponse was infrequent for nearly all data items.

We completed 13,383 12-month interviews, 11,787 30-month interviews, and 11,313 48-month interviews. As Table III.1 shows, the effective response rate to the 12-month follow-up interview was 90.2 percent (91.4 percent program, 88.4 percent control), to the 30-month interview 79.4 percent (80.7 percent program, 77.4 percent control), and to the 48-month interview 79.9 percent (81.5 percent program, 77.8 percent control).[1]

The response rates differed somewhat across some key subgroups. For example, the 48-month interview response rate was higher for females than for males (85 percent, compared to 76 percent) and for those never convicted prior to program application than for those ever convicted (80 percent, compared to 76 percent). Thus, we adjusted the sample weights to help reduce the potential bias in the impact estimates due to interview nonresponse.[2] As with the baseline interview, nonresponse to follow-up interview data items was infrequent.

We completed the average 12-month interview in month 14, and more than three-quarters by month 15 (not shown). Similarly, we completed the average 30-month interview in month 32.5, and

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[1] The effective response rate is the response rate for youths in areas selected for in-person interviews at baseline. This is the relevant response rate for the study, because we did not attempt follow-up interviews with youths who were ineligible for in-person interviews at baseline and who did not complete a baseline interview by telephone within 45 days after random assignment.

[2] The methodological report (Schochet 2001) provides a detailed discussion of interview nonresponse, including the methods used to adjust the sample weights to account for interview nonresponse. This analysis shows that for each research group there are some differences in the average baseline characteristics of respondents to the 48-month interview and the full sample of respondents and nonrespondents. There are fewer differences, however, in the average baseline characteristics of program group respondents and control group respondents.

## TABLE III.1

## EFFECTIVE RESPONSE RATES TO THE 12-MONTH, 30-MONTH, AND 48-MONTH FOLLOW-UP INTERVIEWS, BY RESEARCH STATUS AND KEY SUBGROUP

(Percentages)

| | Effective Response Rate | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 12-Month Interview | | | 30-Month Interview | | | 48-Month Interview | | |
| Subgroup | Program Group | Control Group | Combined Sample | Program Group | Control Group | Combined Sample | Program Group[a] | Control Group | Combined Sample |
| Full Sample | 91.4 | 88.4 | 90.2 | 80.7 | 77.4 | 79.4 | 81.5 | 77.8 | 79.9 |
| Gender | | | | | | | | | |
| Male | 90.8 | 86.8 | 89.1 | 77.9 | 74.3 | 76.3 | 78.2 | 73.7 | 76.2 |
| Female | 92.2 | 91.0 | 91.8 | 84.2 | 82.7 | 83.7 | 85.6 | 84.6 | 85.2 |
| Age at Application | | | | | | | | | |
| 16 to 17 | 92.2 | 90.5 | 91.5 | 81.5 | 79.6 | 80.7 | 81.4 | 79.2 | 80.4 |
| 18 to 19 | 90.9 | 87.6 | 89.6 | 79.9 | 77.4 | 78.9 | 81.9 | 77.3 | 80.0 |
| 20 to 21 | 91.4 | 87.6 | 89.8 | 81.2 | 75.5 | 78.9 | 81.0 | 76.8 | 79.2 |
| 22 to 24 | 90.3 | 84.2 | 87.9 | 79.5 | 72.4 | 76.8 | 81.1 | 75.6 | 78.9 |
| Race/Ethnicity | | | | | | | | | |
| White, non-Hispanic | 89.9 | 87.0 | 88.7 | 80.1 | 77.4 | 79.0 | 80.6 | 78.9 | 79.9 |
| Black, non-Hispanic | 91.8 | 89.4 | 90.9 | 80.7 | 78.0 | 79.6 | 82.3 | 78.6 | 80.8 |
| Hispanic | 91.2 | 85.9 | 89.0 | 80.1 | 75.3 | 78.1 | 79.6 | 73.5 | 76.9 |
| Other | 94.6 | 90.6 | 92.9 | 86.1 | 78.0 | 82.8 | 80.7 | 77.4 | 79.2 |
| Education Level at Application | | | | | | | | | |
| Completed 12th grade | 92.4 | 89.6 | 91.3 | 83.0 | 81.2 | 82.0 | 84.4 | 79.0 | 82.2 |
| Did not complete 12th grade | 91.2 | 88.1 | 89.9 | 80.1 | 76.5 | 78.8 | 80.6 | 77.6 | 79.3 |
| Conviction History at Application | | | | | | | | | |
| Ever convicted or adjudged delinquent | 91.1 | 88.6 | 90.0 | 77.5 | 72.5 | 75.4 | 78.2 | 72.7 | 75.8 |
| Never convicted or adjudged delinquent | 91.4 | 88.3 | 90.1 | 81.0 | 77.6 | 79.6 | 81.6 | 78.2 | 80.2 |
| Residential Designation Status | | | | | | | | | |
| Resident | 91.1 | 87.6 | 89.7 | 80.1 | 76.2 | 78.5 | 81.1 | 76.6 | 82.6 |
| Nonresident | 92.7 | 91.2 | 92.1 | 82.8 | 82.1 | 82.5 | 82.9 | 82.1 | 79.2 |
| **Sample Size in In-Person Areas[b]** | **6,206** | **4,242** | **10,448** | **6,182** | **4,223** | **10,405** | **5,725** | **4,212** | **9,937** |

73

TABLE III.1 *(continued)*

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SOURCE: 12-month, 30-month, and 48-month interview data, and ETA-652 data.

NOTE: The effective response rate is the response rate for sample members eligible for in-person interviews at baseline (that is, those who lived in the in-person areas at application to Job Corps). Youths not in the in-person areas who did not complete baseline interviews by telephone within 45 days after random assignment were not eligible for follow-up interviews.

[a] To reduce data collection costs, 93 percent of program group members eligible for 48-month interviews were randomly selected for 48-month interviewing.

[b] Figures exclude those who died during the follow-up period and 63 cases (31 control group and 32 program group members) in the in-person areas who were determined to have enrolled in Job Corps prior to random assignment and were thus ineligible for the study.

about 78 percent by month 34. Finally, we completed the average 48-month interview in month 49.8, and more than 78 percent by month 51. These figures are similar for program and control group members. Thus, the recall period was similar across sample members and did not differ, on average, by research status.

On the basis of these results, we believe that the interview response rates and data quality are high enough to produce credible impact estimates for the full sample and for key subgroups.

### 3. Analysis Samples

The primary sample used for the analysis includes the 11,313 youths (6,828 program group members and 4,485 control group members) who completed 48-month interviews. About 88 percent of this sample also completed 30-month interviews, and 95 percent completed 12-month interviews. More than 85 percent completed both the 12- and the 30-month interviews, and only 2 percent completed neither. Furthermore, baseline interview data are available for everyone in this sample, because all youths completed either the full baseline interview or the abbreviated baseline interview in conjunction with the 12-month interview.[3] Thus, complete data are available for most of the analysis sample.

The short-term impact report (Schochet et al. 2000) presents impact estimates covering the 30-month period after random assignment using the 11,787 youths who completed 30-month follow-up interviews. These results are very similar to the corresponding estimates covering the 30-month period obtained using the 48-month sample. Thus, we present results covering the entire follow-up period using the 48-month sample only.

The follow-up period for the analysis sample covers the period from November 1994 (the first month after random assignment--month 1--for those randomly assigned in November 1994) to

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[3] About 210 cases in the analysis sample completed an abbreviated baseline interview.

February 2000 (month 48 for those randomly assigned in February 1996). This was a period of strong economic growth. For example, the unemployment rate for the civilian population of those 16 and older was about 5.5 percent in late 1994, about 50 percent in 1997, and about 4 percent in early 2000. Similarly, the unemployment rate for those 16 to 19 decreased from about 17 percent in late 1994 to under 14 percent in early 2000. As discussed in Chapter VI, it is difficult to determine the effects of the strong economy on the impact estimates. However, these potential effects should be kept in mind when interpreting the impact results.

## B. OUTCOME MEASURES

Three criteria guided specification of the major outcome measures for the impact analysis: (1) selecting outcomes that are likely to be influenced significantly by Job Corps participation, (2) selecting outcomes that have policy relevance, and (3) measuring outcomes reliably. Next, we discuss the primary outcome measures, our hypotheses about how they are likely to be affected by Job Corps participation, and their construction. Table III.2 displays the outcome measures used in the analysis.

### 1. Primary Outcome Measures

The primary outcome measures can be grouped into six areas:

***Education and Training.*** The major goal of Job Corps is to provide intensive academic classroom instruction and vocational skills training to increase the productivity, and hence the future earnings, of program participants. The typical Job Corps student stays in the program for an extended period (about eight months on average), and most enroll after leaving school. Thus, participation in Job Corps probably leads to increases in the amount of education and training youths

## TABLE III.2

## OUTCOME MEASURES DEFINED OVER SPECIFIC PERIODS

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**Education and Training**

- All Programs
  - Ever enrolled
  - Number attended
  - Weeks attended
  - Hours per week attended

- Specific Programs
  - Ever enrolled in the following programs: Job Corps; high school; GED; ABE or ESL; vocational, technical, or trade; two-year college; four-year college
  - Weeks attended, by type of program
  - Hours attended, by type of program

- Academic Classes
  - Ever took
  - Weeks took
  - Hours per week took
  - Types of programs where took

- Vocational Training
  - Ever received
  - Weeks received
  - Hours per week received
  - Types of programs where received

- Educational Attainment
  - Degrees, diplomas, and certificates (high school diploma,[a] GED certificate,[a] vocational, technical, or trade certificate or diploma; associate degree; four-year college degree)
  - Highest grade completed

**Employment, Earnings, and Job Characteristics**

- Employment
  - Ever employed
  - Number of jobs
  - Weeks employed
  - Hours per week employed

TABLE III.2 *(continued)*

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**Employment, Earnings, and Job Characteristics (continued)**

Earnings
- Distribution of earnings

Characteristics of the Most Recent Job in Quarter 10 and in Quarter 16
- Had a job
- Months on job
- Usual hours worked per week
- Hourly wage
- Weekly earnings
- Occupation
- Type of employer (private company, military, federal employee, state employee, local government employee, self-employed)
- Job benefits available (health insurance, paid sick leave, paid vacation, child care assistance, flexible hours, employer-provided transportation, retirement pension benefits, dental plan, tuition reimbursement)

Education and Employment Activities
- Ever participated in any activity
- Weeks participated
- Hours per week participated

**Receipt of Public Assistance and Other Sources of Income**

Public Assistance
- Received benefits (AFDC/TANF, food stamps, General Assistance, SSI/SSA, WIC)
- Months received benefits, by type
- Amount of benefits received, by type
- Covered by public health insurance (such as Medicaid) at the 12-, 30-, and 48-month interview
- Lived in a public housing project at the 12-, 30-, and 48-month interview

Other Sources of Income
- Received income (UI child support, from friends, other income)
- Weeks received UI
- Amount received, by type

TABLE III.2 *(continued)*

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**Crime, Alcohol and Illegal Drug Use, and Health**

Criminal Activities

- Ever arrested or charged with a delinquency or criminal complaint
- Number of times arrested
- Months from random assignment until first arrested for those ever arrested
- Most serious charge for which arrested (murder or assault, robbery, burglary, larceny or other property crimes, drug law violations, other personal crimes, other miscellaneous crimes)
- All charges for which arrested
- Convicted, pled guilty, or adjudged delinquent
- Number of times convicted
- Made a deal or plea-bargained
- Most serious charge for which convicted
- All charges for which convicted
- Served time in jail for convictions
- Number of months in jail for convictions
- Put on probation or parole
- Number of times crimes were committed against sample members, by type of crime

Tobacco, Alcohol, and Illegal Drug Use in the 30 Days Prior to the 12-, 30-, and 48-Month Interviews

- Smoked cigarettes
- Consumed alcoholic beverages
- Tried marijuana or hashish
- Snorted cocaine powder
- Smoked crack cocaine or freebased
- Used speed, uppers, or amphetamines
- Used hallucinogenic drugs
- Used heroin, opium, methadone, or downers
- Used other drugs
- Injected drugs with a needle or syringe

Drug and Alcohol Treatment

- In a drug or alcohol treatment program
- Weeks in drug treatment
- Place where treatment was received

Health

- Health status at 12, 30, and 48 months
- At 12, 30, and 48 months, had physical or emotional problems that limited the amount of work or other regular daily activities that could be done
- Type of serious health problem
- Weeks had serious health problem since random assignment

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**Family Formation**

Had children during follow-up period
Number of children had during follow-up period
Had children out of wedlock during follow-up period
Percentage of females pregnant
Had children at 30 and 48 months (including those born before and after random assignment)
Percentage of children living with sample member (for parents)
Percentage of absent children who lived with their other parent[b]
Time spent with children in the past three months[b]
Currently provided support for children (food, child care items, household items, clothing, toys, medicine, babysitting, money)[b]
Gave money in the past month[b]
Gave money occasionally or on a regular basis[b]
Amount of money gave in the past month[b]
Ever used any child care
Type of child care used (child's parent, child's grandparent, other relative, nonrelative, day care center, other)
Weeks used child care
Hours per week used child care
Household membership (living with either parent, another adult relative, adult nonrelatives, or no other adults)
Whether sample member is the head of the household
Number in household
Marital status at 30 and 48 months (never married and not living together; married; living together; separated, divorced, or widowed)

**Mobility**

Distance in miles between zip codes of residence at application to Job Corps and at the 30-month interview
Lived in the same state at application to Job Corps and the 48-month interview
Characteristics of the counties of residence at application to Job Corps and the 48-month interview

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SOURCE: Baseline, 12-month, 30-month, and 48-month interviews.

[a]Outcomes defined only for those who did not have a high school credential at random assignment.

[b]Outcomes defined for those not living with all their children.

receive while enrolled (as measured by increases in hours and weeks received academic classroom instruction and vocational skills training). These increases in education and training could lead to increases in educational attainment (as measured by the receipt of a GED or vocational certificate). Participation in Job Corps may also lead to increases in postsecondary school enrollment (such as two- and four-year colleges, the military, and vocational schools) after Job Corps. Participation in Job Corps, however, is expected to lead to reductions in time spent in alternative programs (such as high school and GED programs outside Job Corps). The effects on high school graduation status, however, are unclear, because about one-fourth of Job Corps centers can grant state-recognized high school diplomas.[4]

***Employment, Earnings, and Job Characteristics.*** The primary hypothesis is that, if all other things are equal, youths who obtain Job Corps education and training will become more productive and, hence, will have greater employment opportunities and higher earnings than those who do not. This increased productivity is expected to enhance employability (as measured by increases in labor force participation, employment, hours worked per week, and the proportion of weeks worked) and to increase wage rates, earnings, and fringe benefits available on the job. Furthermore, because the Job Corps program provides placement assistance to participants when they leave the program, program group members should be more likely than control group members to find jobs and to find jobs that match their skills.

We expect, however, that Job Corps participation will reduce employment and earnings during the period of enrollment, because some participants would hold jobs if they had not gone to Job Corps. However, as program participants finish their participation, we expect employment and earnings to rise after a period of readjustment. In light of the variation in the duration of program participation, it is difficult to predict how long after random assignment positive employment and earnings gains will emerge.

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[4] Job Corps participation could also lead to improvements in literacy skills, either directly, through participation in Job Corps basic education, or indirectly, by causing more students than would otherwise have done so to engage in skill-enhancing activities like work and further schooling. Program impacts on participants' literacy skills are presented in Glazerman et al. (2000).

***Receipt of Public Assistance and Other Sources of Income.*** A set of hypotheses closely related to labor market activities involves the effects of the Job Corps program on welfare dependence. Job Corps participants may experience a reduction in welfare receipt while they are in the program (to the extent that they would have been recipients were they not in the program). In addition, because their postprogram earnings may increase, they are expected to receive fewer public transfers (including Aid to Families with Dependent Children [AFDC] or Temporary Assistance for Needy Families [TANF], General Assistance [GA], food stamps, and Special Supplemental Food Program for Women, Infants and Children [WIC]).

***Crime, Alcohol and Illegal Drug Use, and Health.*** Job Corps seeks to help youths become more employable and productive citizens. An important aspect of this process is to teach civic awareness and respect for others. In addition, many enrollees leave their neighborhoods to attend Job Corps. Thus, Job Corps is expected to reduce the incidence and severity of crimes committed by program participants (as measured by the number of arrests and convictions, the types of crimes committed, and the time spent in jails and on probation). While students are enrolled in the program, reductions in criminal activities should be pronounced, because Job Corps participants' activities are restricted, their behavior is monitored, and their material needs are met. Furthermore, most are isolated from social and environmental pressures to engage in criminal activities. After they leave the program, reductions in crime measures are expected to continue, but at a lower rate.

Job Corps should also lead to a reduction in crimes committed against Job Corps students. While at Job Corps centers, youth are less exposed to criminals who would victimize them. In addition, if, after they have left Job Corps, students relocate to safer neighborhoods or spend less time hanging out on the street, the incidence of crimes committed against them may also be lower.

Job Corps is also expected to reduce participants' drug and alcohol use, both during and after the program. While youths are enrolled, impacts on drug and alcohol abuse should be pronounced, for two reasons. First, Job Corps forbids the use of these substances at centers, and behavior is closely monitored. Second, Job Corps provides some drug and alcohol abuse treatment. In the postprogram period, reductions in drug and alcohol use are expected to continue, because Job Corps should have a positive impact on attitudes toward it. Psychological and financial benefits derived from the program may also induce participants to feel more hopeful and under less pressure to use these substances.

Participation in Job Corps is also expected to increase participants' overall health status, for reasons similar to those discussed earlier, and because the program offers comprehensive health services and health education.

***Family Formation.*** Important dimensions of personal responsibility are relationships with the opposite sex and the decision to have and raise children. The Job Corps program recognizes the importance of this area by requiring all students to take education program units on social and emotional well-being, sexuality, and parenting. Perhaps more important, other aspects of center experience, as well as improved economic opportunities resulting from Job Corps participation, may lead to changes in the way a youth relates to the opposite sex and on decisions to bear and raise children. Thus, the study examines a series of six outcomes related to family formation and children: (1) the likelihood of marriage; (2) the likelihood of forming a stable, long-term relationship with a single partner; (3) the likelihood of bearing or fathering children while unmarried; (4) the likelihood of living with one's children and the level of involvement with child rearing; (5) the nature and extent of financial and nonfinancial support for absent children; and (6) the use of child care services.

***Mobility.*** Many youths served by Job Corps live in neighborhoods where poverty rates are high and job opportunities are scarce. A core element of the philosophy motivating the residential component of Job Corps is that, for some, insurmountable barriers to succeeding in training in the youth's environment require removal from the home. Indeed, living in a debilitating environment that precludes participation in other education and training programs is a key Job Corps eligibility criterion.

This element of Job Corps raises the question of whether participation promotes mobility of students. Participation in Job Corps could affect the types of areas where students live after they leave the program, because of job placement and location assistance and because of the higher earnings that could make some neighborhoods more affordable. Thus, we examine the extent to which students return to the same areas that they lived in at the time of application, and the characteristics of the areas that they lived in at the 48-month interview.

### 2. Construction of Outcome Measures

Our analytic approach for the impact analysis focused on estimating period-specific impacts (that is, differences in outcomes between program and control group members by period). We constructed period-specific outcome measures using information on the dates that events occurred.[5] For example, we constructed timelines to determine whether a sample member was working or in school or training in a given week or was receiving various types of public assistance (such as AFDC/TANF or food stamps) in a given month. As another example, we used self-reported crime data to determine the timing of arrests and used fertility information to determine the timing of births. We also constructed period-specific measures about the characteristics of each activity. For example, we constructed measures of sample members' earnings, number of hours worked or in school, degrees received, public assistance benefit levels, and types of arrest charges over a given period.

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[5] A methodological appendix (Schochet 2001) provides a detailed discussion of the construction of outcome measures, including the treatment of missing values and outliers.

Outcome measures were defined for the following periods after random assignment: (1) each quarter, (2) each year, and (3) the entire 48 months. The quarterly measures were used to examine changes in impact estimates over time and were constructed for key employment- and education-related outcomes. We used the yearly measures to summarize activities during the "in-program" and "postprogram" periods for many outcomes. As described in Chapter IV, the first year after random assignment was a period of intensive Job Corps participation for those in the program group who enrolled in centers, and the second year was a period of still significant but less intensive Job Corps participation. The last two years during the 48-month period were largely a postprogram period, because most program group members were no longer enrolled in Job Corps. We also constructed outcome measures that summarized sample member experiences over the entire 48-month period.

Some outcome measures pertain only to the time of the interview. For example, the follow-up interviews gathered data about tobacco, alcohol, and illegal drug use in the past 30 days and obtained information on the respondent's highest grade completed, overall health status, address, and living arrangements at the time of the interview.

## C. ANALYTIC METHODS

The random assignment design ensures that no systematic observable or unobservable differences between program and control group members existed at the point of random assignment, except for the opportunity to enroll in Job Corps. Thus, simple differences in the distributions of outcomes between program and control group members are unbiased estimates of program impacts for eligible applicants.

Two important points about the interpretation of these impact estimates warrant discussion. First, as noted earlier, these impact estimates represent the effects of Job Corps relative to other employment and training programs in the community, and not relative to no training. Thus, the impact estimates represent the *incremental* effect of Job Corps relative to other programs in which control group members participated. Consequently, in order to interpret the impact estimates, it is crucial to examine the employment and training experiences of control group members to understand the "counterfactual" for the evaluation.

Second, the comparison of the outcomes of all program and control group members yields *combined* impact estimates for the 73 percent of program group members who enrolled in Job Corps centers and the 27 percent who did not. Policymakers, however, are more concerned with the effect of Job Corps on those who enrolled in a center and received Job Corps services. This analysis is complicated by the fact that the straightforward comparison of the outcomes of Job Corps participants in the program group and all control group members does not yield the desired impact for program participants. Ideally, we would like to compare the outcomes of program group participants with control group members who would have shown up at a center had they been in the program group. However, we cannot identify these control group members. Nevertheless, as discussed in these sections, we can overcome these complications if we assume that Job Corps has no impact on eligible applicants who do not enroll in centers.

In this section, we discuss our analytic approach for estimating impacts per eligible applicant and per Job Corps participant only, for the full sample and for key population subgroups. In addition, we discuss our approach for adjusting the impact estimates for the small number of control group members who enrolled in Job Corps. Finally, we discuss how the results are presented and interpreted.

### 1. Estimating Impacts per Eligible Applicant

We obtained the estimates of Job Corps impacts per eligible applicant by computing differences in average outcomes between all program and control group members (that is, using a differences-in-means approach). This approach yields unbiased estimates of the effect of Job Corps for program applicants who were determined to be eligible for the program. We used the associated t-tests (for variable means) and chi-squared tests (for distributions of categorical variables) to test the statistical significance of the impact estimates. We conducted the analysis using the 11,313 youths (6,828 program group members and 4,485 control group members) who completed 48-month interviews. We calculated all figures using sample weights to account for the sample and survey designs and for the effects of interview nonresponse, so that we could generalize the estimates to the intended study population. Standard errors of the estimates account for design effects due to unequal weighting of the data and to clustering caused by the selection of areas slated for in-person interviewing at baseline.[6]

We also estimated "regression-adjusted" impact estimates using multivariate models that control for other factors measured at baseline that affect the outcome measures. This approach increases the precision of the estimated program impacts and the power of significance tests relative to the differences-in-means approach. In addition, the use of multivariate models can adjust for any random residual differences in the observable baseline characteristics of program and control group members.

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[6] The report containing methodological appendixes (Schochet 2001) describes the construction of sample weights and standard errors.

Obtaining unbiased impact estimates using the regression approach, however, is computationally difficult because of the study's complex sample and survey designs, which generated a large number of strata (weighting cells). As discussed in more detail in Schochet (2001), the usual procedure of regressing outcomes on a program status indicator variable (which is 1 for program group members and 0 for control group members) and other explanatory variables can yield biased estimates of program impacts (that is, biased coefficient estimates on the program status indicator variable) because the estimates may be "weighted" incorrectly. Furthermore, estimating weighted regressions does not solve the problem (DuMouchel and Duncan 1983). To obtain unbiased impact estimates, separate regression-adjusted estimates must be obtained in each of the 48 weighting cells (many of which contain only a small number of sample members), and the weighted average of these 48 separate estimates must be calculated. Having small numbers of sample members in some weighting cells necessitates aggregating across weighting cells, which could introduce some bias if impacts differ across the weighting cells.

The results obtained using the differences-in-means approach and the regression approach are similar, and the same policy conclusions can be drawn from both sets of estimates (Schochet 2001). We present the differences-in-means estimates in this report for several reasons. The gains in precision from the regression approach are small for most outcome measures and subgroups. In addition, we can be sure that the differences-in-means estimates are unbiased (because sample weights can be used in this context to account for the sample design and interview nonresponse) and relatively precise (because the samples are large). Finally, few differences existed in the average baseline characteristics of program and control group members, so controlling for these differences does not change the impact estimates materially.

We also present program and control group differences for some outcomes that are conditional on other outcomes. For example, we compared hourly wage rates and fringe benefits received on the most recent job for program and control group members who worked in months 46 to 48. As another example, we compared the financial support provided by program and control group members to their children who did not live with them. These estimates may not be unbiased estimates of program impacts, because they are based on potentially nonrandom subsets of program and control group members (that is, those who worked or were noncustodial parents). The baseline characteristics (both measured and unmeasured) of those in these subsets may have differed by research status because of potential program effects on the composition of youths in the subsets. However, these comparisons provide important insights into the differences between the outcomes of program and control group members.

### 2. Estimating Impacts per Job Corps Participant

Program impact estimates for program group members who enrolled in Job Corps--*participants*--were obtained by dividing the program impact estimates per eligible applicant by the proportion of program group members who enrolled (Bloom 1984). To illustrate how this works, we can express the impact of the Job Corps program per eligible applicant as a weighted average of the program impact for those eligible applicants who would enroll in Job Corps, given the chance, and the program impact for those eligible applicants who would not enroll, with weights $p$ and $(1 - p)$, where $p$ is the proportion of eligible applicants who enroll (73 percent).[7] We do not know which control group members would have enrolled if they had been assigned to the program group, or which control group members would not have enrolled. However, this information is not necessary if we assume that all impacts for the full program group were due to those who showed up at a center, and that *the impacts on no-shows are zero*. With this assumption, the impact per eligible applicant reduces to $p$ times the impact per participant. Thus, the impact per participant can be computed by dividing the impact estimates based on *all* program and control group members by the proportion of program group members who actually enrolled in a center.[8]

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[7] In mathematical terms, $I_E = p*I_S + (1-p)*I_{NS}$, where $I_E$ is the impact on eligibles, $I_S$ is the impact on those who showed up at a center (that is, the difference between the average outcomes of program
(continued...)

The key assumption that makes this procedure work is that the program has no effect on no-shows. Although this assumption is reasonable, the offer of a Job Corps slot might affect the behavior of eligible applicants who do not enroll at a center. For example, after being determined eligible for Job Corps, no-shows might alter their job search behaviors because they have the option of enrolling. In particular, reservation wages might increase relative to what they would have been if a youth did not have the opportunity to enroll in Job Corps. Although it is unlikely that the offer of a Job Corps slot without active participation will have an appreciable effect on long-term outcome measures, it may have an effect on job search and employment in the short term. These issues are explored further in a separate report (Gritz et al. 2001).

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[7](...continued)
group participants and control group members who would have participated if given the chance), and $I_{NS}$ is the impact on no-shows (that is, the difference between the average outcomes of program group no-shows and control group members who would been no-shows if they were in the program group).

[8] The standard error of the impact estimate for participants was inflated to account for the estimation error in the show rate (Schochet 2001).

### 3. The Adjustment for Crossovers in the Control Group

About 1.4 percent of all control group members (and 1.2 percent of control group members in the 48-month sample) enrolled in Job Corps before their three-year restriction period ended. We refer to these youths as "early crossovers." In addition, 3.2 percent of control group members in the 48-month sample enrolled in Job Corps between three and four years after random assignment (that is, after their restriction period ended). We refer to these youths as "late crossovers." Crossovers were treated as control group members in the analysis to preserve the integrity of the random assignment design. Thus, impact estimates that do not account for these crossovers could be biased. Next, we discuss our approach for adjusting the impact estimates for early and late crossovers.

#### a. The Adjustment for Early Crossovers

A small number of control group members enrolled in Job Corps before their three-year embargo period ended. As described in the report on study implementation (Burghardt et al. 1999), the Job Corps national office allowed most of these youths to remain at centers, but held OA and center staff accountable for these errors. The average duration of stay in Job Corps for these youths (7.6 months) was very similar to the average duration of stay for program group enrollees (8.0 months). Thus, impact estimates on employment and earnings in the postprogram period that do not adjust for these crossovers could be slightly biased downwards if these crossovers benefited from participation in Job Corps.

The procedure to estimate impacts per participant can be extended to accommodate early control group crossovers (Angrist et al. 1996). As described in Schochet (2001), the modified procedure involves dividing the estimated impacts per eligible applicant by the difference between the Job Corps enrollment rate for the program group (73 percent) and the early crossover rate for the control group (1.2 percent). These impacts pertain to eligible applicants who would enroll in Job Corps if they were assigned to the program group, but who would not enroll if they were instead assigned to the control group. Thus, the impacts pertain to a subset of all participants.[9] However, because the crossover rate is very small, the adjustment procedure has very little effect on the estimates.

### b. The Adjustment for Late Crossovers

Control group members were allowed to enroll in Job Corps after their three-year restriction period ended. About 3.2 percent of control group members enrolled in the program between their third and fourth years after random assignment. The enrollment rate was 4.6 percent for those 16 and 17 at application to Job Corps, 2.7 percent for those 18 and 19, and 1.1 percent for those 20 to 24. About 55 percent of these late crossovers were enrolled in Job Corps during the last quarter of the four-year period.

The approach to accommodate the *early* crossovers cannot be used to accommodate the *late* crossovers. The adjustment procedure for *early* crossovers assumes that the average outcomes of early crossovers in the control group were the same as those in the program group who would have been early crossovers had they instead been assigned to the control group (whom we label "would-be" early crossovers). This assumption is reasonable, because most early crossovers in the control group enrolled in Job Corps soon after random assignment and thus were in Job Corps at roughly the same time as the would-be early crossovers in the program group. Thus, it is likely that average earnings during the postprogram period were similar for the two groups.

The *late* crossovers, however, enrolled in Job Corps more than three years after random assignment, whereas nearly all program group participants enrolled within one year. Thus, we cannot assume that the average outcomes of late crossovers in the control group were similar to those of would-be late crossovers in the program group. Instead, average earnings late in the observation period were probably much lower for the late control group crossovers than for their program group counterparts, because more than half these control group members were enrolled in Job Corps during this period, and those who had left Job Corps had been out for only a short period. Consequently, impact estimates on postprogram employment and earnings that do not adjust for these late control group crossovers would probably be biased slightly upwards.

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[9] In the literature, these impacts are referred to as impacts per "complier." However, we sacrifice technical accuracy for clarity and refer to them as impacts per participant.

Our procedure to adjust for the late control group crossovers was to "assume" that these crossovers never enrolled in Job Corps, and to impute their employment and education outcomes covering the last five quarters of the 48-month period. We conducted the imputation procedure in two stages. In the first stage, we identified noncrossovers in the control group whose average demographic characteristics and employment and education experiences during the first two years after random assignment were similar to those of the late crossovers.[10] Second, we imputed the employment and education outcomes of late crossovers using the average outcomes of noncrossovers in the matched sample (by age and gender).[11]

### 4. Subgroup Analysis

Program impact estimates for the full sample may conceal important differences in impacts across subgroups of program participants. If impacts do exist overall, they might be heavily concentrated in or much larger for some subgroups. Conversely, if impacts do not exist overall, they might exist for some subgroups. If a subgroup is small, the impact on it might not be large enough to yield a statistically significant difference in the overall sample.

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[10] We used propensity score procedures to select the matched sample. The probability that a control group member was a late crossover was regressed on a set of explanatory variables, and a predicted probability (propensity score) was calculated for each control group member. We then selected the matched sample of noncrossovers as those with the closest propensity scores to those of the crossovers.

[11] We did not impute other outcomes (such as crime, welfare, and family formation measures) for the late crossovers.

This report addresses two important questions about impacts for subgroups:

1. Is Job Corps more effective for some groups of youths defined by personal characteristics or experiences before program application than for other groups?
2. Are the residential and nonresidential components effective for the students they serve?

### a. Subgroups Defined by Youth Characteristics

It is important to identify groups of Job Corps students who benefit from program participation, so that policymakers can improve program services and target them appropriately. In consultation with the study advisory panel (which included representatives of Job Corps), we identified groups of students whose backgrounds, training needs, and program experiences typically differ in important ways. The selected groups often enroll in different types of centers and program components, and they experience a different mix of vocational skills and academic classroom training while enrolled.

Using baseline interview data, we estimated program impacts on seven sets of subgroups defined by youth characteristics at random assignment:[12]

1. ***Gender.*** The training needs and the barriers to successful employment of young women who enroll in Job Corps are different from those of young men who enroll. As discussed in Chapter II, the average characteristics of female students differ from those of male students (for example, female students tend to be older, to have completed high school, and to have children). In addition, female students are more likely to be nonresidential students and are less likely to be in CCCs. Thus, in light of the different programmatic needs and program experiences of males and females, an important policy issue is the extent to which Job Corps is effectively serving each of these groups.
2. ***Age at Application to Job Corps.*** The broad age range Job Corps serves means that the program must serve adolescents and young adults together. This poses a significant challenge for the program, because the training needs and backgrounds of younger

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[12] Appendix Table A.1 displays sample sizes for the subgroups.

students differ from those of older students. For example, younger students tend to have lower education levels (and thus are much more likely to require education services in Job Corps), less work experience, and fewer children. In addition, younger students exhibit some characteristics (for example, higher arrest rates and incidence of drug use) that suggest that they may be more disadvantaged than older applicants. Moreover, findings from the process analysis reveal widespread concern among Job Corps staff that the younger students are often disruptive and harder to serve than the older students. Thus, an important policy objective is to assess whether Job Corps participation improves the outcomes of these relatively diverse groups. Separate impact estimates are presented for those (1) 16 and 17 years old, (2) 18 and 19 years old, and (3) 20 to 24 years old.[13]

3. ***Educational Attainment.*** Approximately 8 out of 10 Job Corps students lack a GED or high school diploma at the time of entry. Most students without a high school credential begin their Job Corps program with a balanced schedule of one-half academic course work and one-half vocational course work. These students do not normally focus on their vocational trades until they receive their GEDs; hence, most receive intensive academic education while in the program. On the other hand, students with a high school credential usually complete their academic requirements quickly and move toward a full-time vocational schedule. In light of the differences in the mix of vocational and academic classroom experiences in Job Corps and in the characteristics of those with and without a high school credential, we present separate impact estimates for each group.

4. ***Presence of Children for Females.*** The barriers to successful employment for female Job Corps enrollees with children are particularly acute. At application to Job Corps, females with children (who represent about 30 percent of all female students) are highly dependent on public assistance (for example, about 70 percent of these mothers received AFDC/TANF benefits or were part of families that received these benefits in the previous year) and have lower earnings and employment rates than other students. Furthermore, these young mothers are much less likely to live with other adults than other students, suggesting that many lack adequate support systems. Many have problems establishing suitable child care arrangements. Consequently, an important policy issue is the extent to which Job Corps can increase employment and earnings and reduce the chances that these youth become reliant on public assistance.

   In addition, a large percentage of females with children are in the nonresidential component. For example, nearly 65 percent of females with children in our sample were designated for nonresidential slots, and nearly half of all nonresidential designees were females with children. Thus, policy concerns about the effectiveness of the nonresidential program and increasing the recruitment of young females are linked to the effectiveness of Job Corps in serving females with children. Thus, separate impact estimates are presented for females with and without children.

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[13] The age categories were defined in this way because the factors associated with enrolling in a center and graduating from the program were similar for program group members within each group (Johnson et al. 2000).

5. ***Arrest History.*** To be eligible for Job Corps, applicants must be free of behavioral problems that would prevent them from adjusting to the Job Corps standards of conduct. Job Corps seeks to offer youths who may have been in trouble with the law the opportunity to turn their lives around. On the other hand, an applicant cannot currently be under the control of the criminal or juvenile justice system. Furthermore, the program is not equipped to handle youths who pose a threat of violence to themselves or others. Thus, youths with prior involvement with the criminal justice system are carefully screened by the OA agency and sometimes by the regional office.[14]

   The baseline data indicate that over one-quarter of eligible applicants were ever arrested or charged with a delinquency or criminal complaint, and that about five percent were charged with serious crimes, such as aggravated assault, murder, robbery, or burglary. Consequently, an important policy question is the extent to which Job Corps can effectively serve those with previous problems with the law, especially under the new strict ZT policies. In the analysis, we obtained separate impact estimates for those who were (1) never arrested, (2) ever arrested for nonserious crimes only, and (3) ever arrested for serious crimes.

6. ***Race and Ethnicity.*** The backgrounds of Job Corps students differ markedly by race and ethnicity. Whites are more likely than other groups to be male (67 percent, compared to about 56 percent for other groups). Whites tend to have had more work experience, even though the age distribution is similar by race and ethnicity. In addition, whites are less likely to have children, to have received public assistance in the prior year, or to be high school dropouts.

   Program experiences are also likely to differ by race and ethnicity. There are large differences in the racial and ethnic composition across regions (and across centers within regions), and Job Corps operations differ somewhat across regions. For example, about 60 percent of eligible applicants in Regions 2, 3, 4, and 5 are African American, whereas most youths in Regions 1, 7/8, and 10 are white. More than one-third of youths are Hispanic in Regions 2, 6, and 9. Furthermore, whites are much more likely to be in CCC slots and much less likely to be in the nonresidential component. Thus, differences in background characteristics and program experiences by race and ethnicity could lead to differences in program impacts across these groups. Four subgroups defined by race and ethnicity were used in the analysis: (1) white, non-Hispanic; (2) African American, non-Hispanic; (3) Hispanic; and (4) other (including American Indian, Alaskan Native, Asian, and Pacific Islander).[15]

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[14] Findings from the process analysis indicate that nearly all OA counselors (accounting for 96 percent of applicants) require local criminal justice records of all applicants.

[15] Sample sizes for American Indians, Alaskan Natives, Asians, and Pacific Islanders were too (continued...)

7. ***Job Corps Application Date and the New Job Corps Policies.*** As discussed, in response to congressional concerns about the operation of the Job Corps program, new ZT policies were instituted in March 1995--during the sample intake period for the study. The process analysis found that the new policies had a profound positive effect on behavior management and the general climate at centers.[16] Thus, to assess the extent to which the new policies had an effect on program impacts, we present separate impact estimates for those who applied to Job Corps before and after March 1, 1995.[17] Because the ZT policies are still in effect, the post-ZT estimates are more likely to be representative of the current Job Corps program.

We also estimated program impacts for finer subgroups formed by combining groups across these seven categories. This analysis was conducted to help disentangle the subgroup findings, because many of the subgroups are correlated with each other. For example, nearly all those 16 and 17 years old did not have a high school credential at random assignment, compared to 50 percent of those 20 or older. Thus, impact estimates for those without a high school credential are heavily weighted by the outcomes of the younger sample members. Consequently, we obtained separate impact estimates for the younger dropouts and the older dropouts to better understand the extent to which Job Corps helps those with low levels of education.

This finer subgroup analysis was often limited by small sample sizes, which sometimes led to unstable results. However, the analysis provided important insights about the pattern of program effects across key subgroups.

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[15](...continued)
small to support separate impact estimates for these groups.

[16] The policies, however, did not appear to have a significant effect on the characteristics of eligible applicants (Schochet 1998a).

[17] Program group members in the pre-ZT group who were in Job Corps after March 1, 1995, were subject to the new rules. Thus, impact estimates pertaining to the pre-ZT period are somewhat contaminated. Furthermore, program experiences could differ by season, and because of the limited sample intake period, the data are not available to compare impacts for those in pre-ZT and post-ZT groups who were recruited during the same time of year. Thus, differences in the pre-ZT and post-ZT impact estimates are only suggestive of the effects of the new policies.

We view the subgroups defined by age, gender, and the presence of children (for females) as particularly important (along with the results for residents and nonresidents). Thus, in the report, we usually emphasize impact findings for these subgroups more heavily than for other subgroups. However, the emphasis we place on various subgroups varies somewhat, depending on the outcome measure and our hypotheses about the extent and nature of program impacts. For example, when examining impacts on education and training outcomes, we emphasize subgroups defined by age and high school credential status at baseline, because of differences in the educational needs and the expected academic classroom and vocational training experiences of both program and control group members across these subgroups. Similarly, we focus on subgroups defined by gender and the presence of children (but not age) when examining impacts on the receipt of public assistance benefits, because of large differences in the types and amounts of assistance that these gender groups typically receive. As a final example, we focus on age and gender subgroups when examining impacts on crime-related outcomes, because of subgroup differences in the level of involvement with the criminal justice system, but we do not focus on the results for females with and without children, because we had no reason to believe that crime-related impacts would differ for these two groups of females.

**Estimation Issues.** The random assignment design ensures that unbiased impact estimates for a subgroup defined by a youth characteristic can be obtained by comparing the distribution of outcomes of program and control group members in that subgroup. Thus, for example, impact estimates for males were obtained by comparing the outcomes of male program and control group members. Similarly, impacts estimates for those without a high school credential were computed by comparing the outcomes of program and control group members without a high school credential at random assignment.

Standard statistical tests were used to gauge the statistical significance of the subgroup impact estimates. In addition, we conducted statistical tests to determine whether program impacts were similar across levels of a subgroup. For example, we tested the hypothesis that program effects were similar for males and females and across the three age groups.

### b. Impacts for Residents and Nonresidents

Residential living is the component that distinguishes Job Corps from other publicly funded employment and training programs. During our site visits to centers as part of the process analysis, staff stressed the importance of the residential component as central to helping students become more employable. Some staff believe that it is even more important than vocational training for improving the long-term outcomes of students. However, staff also stressed that the nonresidential component is important because it serves a type of student different from those in the residential component, and because nonresidents, who have outside commitments to families or children, might not enroll in Job Corps if a nonresidential option were not available.[18] About 12 percent of enrollees in the study program group were nonresidents.

The process analysis found that nonresidential students are fully integrated into the academic and vocational components of Job Corps, and receive comprehensive and intensive services. However, the participation of many nonresidential students in other activities is limited, often because of family responsibilities. For example, nonresidential students are less involved in dormitory life, student government, and recreational activities. Thus, nonresidential students have a program experience that may differ from that of students who live on center.

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[18] Most centers have some nonresidential slots, and about 25 percent of centers have at least 20 percent of their slots reserved for nonresidential students.

The estimation of separate impacts for those in the residential and nonresidential components is of considerable policy interest for two reasons. First, as discussed, the residential and nonresidential components serve students with different characteristics and needs, and program experiences may differ by residential status. Second, previous studies (for example, the JTPA and JOBSTART evaluations) have found that disadvantaged youths do not benefit significantly from participation in training programs that offer basic education and job-training services in a nonresidential setting. Thus, there is great interest in measuring impacts of Job Corps on nonresidential students, to help guide design decisions not only about Job Corps, but also about other programs to support youths' labor market participation.

However, the Job Corps nonresidential component is very different from most other nonresidential training programs. As discussed, nonresidential students in Job Corps receive services that are similar in many ways to those received by residential students. In fact, the program cost per nonresidential student is only about 12.5 percent less than the program cost per residential student (McConnell et al. 2001). Thus, the nonresidential Job Corps program is more intensive and comprehensive, and hence, more expensive, than most other nonresidential programs. Furthermore, unlike most other nonresidential programs, nonresidential and residential students in Job Corps train together, because most centers with nonresidential slots also have residential slots. Thus, nonresidential Job Corps students may benefit from their contact with residential students. These qualifications suggest that we must proceed with caution when comparing impact results for nonresidential students in Job Corps and in other programs.

**Estimation Issues.** We estimated the impacts of the residential and nonresidential components using data on OA counselor predictions as to whether sample members would be assigned to a residential or a nonresidential slot. As part of the application process, OA counselors filled in this information on a special form (an ETA-652 Supplement form) developed for the study. OA staff sent these forms to MPR for those youths determined to be eligible for the program, and MPR entered the information into the study's database.

The anticipated residential status information is available for both program *and* control group members because it was collected prior to random assignment. Thus, we estimated the impacts of the residential component by comparing the distribution of outcomes of program group members designated for a residential slot with those of control group members designated for a residential slot. Similarly, we estimated the impacts of the nonresidential component by comparing the experiences of program and control group members designated for nonresidential slots. We used standard statistical tests to gauge the statistical significance of these impact estimates.

We believe that the analysis produced reliable estimates of program impacts for the residential and nonresidential components, because the anticipated residential status information is available for all sample members and matches actual residential status very closely. Because it was a key data item required for random assignment, the anticipated residential status information is available for all sample members. If the information was missing, MPR contacted OA staff and did not perform random assignment until it was provided.

OA counselor projections of residential status proved to be very accurate (Schochet 1998b). Using SPAMIS information on program group members who enrolled in centers, we found that about 98 percent of program group enrollees designated for residential slots actually enrolled in them and about 88 percent of program group enrollees designated for nonresidential slots actually enrolled in those.[19] Moreover, the accuracy of the predictions was high across all key subgroups. Thus, the experiences of those designated for residential (nonresidential) slots were largely representative of the experiences of actual residents (nonresidents), and vice versa.[20]

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[19] In addition, a large proportion of program group members who enrolled in a particular component were designated for that component. For example, more than 98 percent of all enrollees in residential slots were designated for these slots, and about 84 percent of those in nonresidential slots were designated for those slots.

An important (yet subtle) point about the interpretation of the impact findings for residents is that they tell us about the effectiveness of the residential component *for youths who are typically assigned to residential slots* (because the results were obtained by comparing the outcomes of program and control group members who were suitable for the residential component). Similarly, the impact estimates for nonresidents tell us about the effectiveness of the nonresidential component *for youths who are typically assigned to nonresidential slots*. The results cannot necessarily be used to measure the effectiveness of each component for the *average* Job Corps student.[21] Nor can the results be used to assess how a youth in one component would fare in the other one.

These important qualifications can be understood further by noting that the characteristics of residential and nonresidential designees differ in important ways (see Table III.3, which presents key

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[20] We attempted to improve the accuracy of the "predictions" by using multivariate techniques. We estimated logit models where the probability that a program group enrollee was assigned to the residential component was regressed on the predicted assignment measure and other explanatory variables created using baseline interview data. We then used the parameter estimates from these models to create predicted probabilities for *all* control group and program members. The sample was then split into those likely to be residents (those with high predicted probabilities) and those likely to be nonresidents (those with low predicted probabilities). We then conducted the analysis using these groups. The models did not increase the accuracy of the predictions appreciably, and the results using the multivariate procedure were similar to those obtained with the anticipated assignment information only.

[21] To address this question effectively, we would have had to randomly assign each youth in the study population to the residential or nonresidential component. We rejected this design option because it would have introduced an unacceptable degree of intrusion into normal program operations.

## TABLE III.3

## BASELINE CHARACTERISTICS OF RESIDENTIAL AND NONRESIDENTIAL DESIGNEES IN AREAS WITH A LARGE CONCENTRATION OF NONRESIDENTIAL STUDENTS, BY GENDER

(Percentages)

| | Females | | Males | |
|---|---|---|---|---|
| Baseline Characteristic | Residential Designees | Nonresidential Designees | Residential Designees | Nonresidential Designees |
| Age at Application | | | | |
| 16 to 17 | 50.7 | 24.4 | 48.3 | 31.4 |
| 18 to 19 | 28.7 | 32.3 | 26.9 | 35.4 |
| 20 to 24 | 20.7 | 43.3 | 24.7 | 33.2 |
| Had Children | 16.5 | 64.7 | 9.8 | 18.7 |
| Race/Ethnicity | | | | |
| White, non-Hispanic | 12.1 | 9.6 | 15.9 | 15.5 |
| Black, non-Hispanic | 60.6 | 68.7 | 59.5 | 55.1 |
| Hispanic | 23.6 | 17.5 | 19.3 | 20.9 |
| Other | 4.3 | 4.2 | 5.3 | 8.5 |
| Had a High School Diploma or GED | 21.3 | 34.0 | 17.1 | 24.5 |
| Received Welfare in the Past Year[a] | 67.7 | 78.4 | 56.2 | 60.6 |
| Had a Job in the Past Year | 62.0 | 52.8 | 59.5 | 63.9 |
| Was Ever Arrested | 15.6 | 12.3 | 30.3 | 26.8 |
| **Sample Size** | **873** | **1,312** | **1,357** | **445** |

SOURCE: Baseline interview data and Supplemental ETA-652 data for those who completed 48-month interviews.

NOTE: Figures pertain to those who lived in one of the 57 areas sending the largest number of nonresidential students to Job Corps. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

[a] Welfare receipt includes AFDC/TANF, food stamps, or other public assistance.

baseline characteristics by residential designation status and gender in areas with large concentrations of nonresidential students). For both males and females, nonresidential designees are much more likely than residential designees to be older, to have children, and to have a high school credential, and are less likely to have ever been arrested. Thus, the residential and nonresidential program components serve very different students, and our design can address only the extent to which each component effectively serves students suited for it.

Our analysis findings suggest that there are some differences in the impact estimates for residents and nonresidents by gender and, for females, by the presence of children. Thus, we focus on these finer subgroup results in the report.

### 5. Presentation of Results

We present analysis findings using a series of figures, charts, and tables. The tables (which form the basis for the figures and charts) display the following seven pieces of information for each outcome measure:

1. ***The Control Group Mean for Eligible Applicants.*** This figure was calculated using the entire control group and represents the mean outcome of program group members if they had not been offered a Job Corps slot.

2. ***The Program Group Mean for Eligible Applicants.*** We calculated this mean using the full program group (participants and no-shows).

3. ***The Impact Estimate per Eligible Applicant.*** This estimate is the difference between the mean outcomes for program and control group members.

4. ***The Mean for Program Group Members Who Participated in Job Corps.*** This mean was used to examine the outcomes of program group members who enrolled in Job Corps (and who would not have enrolled in Job Corps if they had instead been assigned to the control group).[22]

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[22] The qualification in parentheses results from our approach for adjusting the impacts to account for the small number of early crossovers in the control group, as discussed earlier in this section.
(continued...)

5. ***The Impact Estimate per Program Participant.*** This estimate is the impact estimate per eligible applicant divided by the difference between the program group participation rate in Job Corps (73 percent) and the control group early crossover rate (1.2 percent). The participation and crossover rates differed somewhat across subgroups.

6. ***The Percentage Gain Due to Participation in Job Corps.*** This estimate represents the percentage change in the mean outcome for participants relative to what it would have been if the participants had not enrolled in Job Corps. The figure is estimated by dividing the impact estimate per program participant by an estimate of the mean for control group members who would have enrolled in Job Corps if they had instead been assigned to the program group (and who were not crossovers). This control group mean was estimated as the difference between the mean for program group participants and the impact estimate per participant.

7. ***An Indication of the Statistical Significance of the Impact Estimates.*** Two-tailed statistical tests were performed to test the null hypothesis of no program impact. We indicate whether the null hypothesis was rejected (that is, whether the impact is statistically significant) at the 1 percent, 5 percent, or 10 percent level. Standard errors used in these test statistics were adjusted for design effects due to unequal weighting and clustering of the in-person sample at baseline. The standard errors of the estimated impacts per participant were also inflated to account for the estimation error in the Job Corps enrollment rate. For the subgroup analysis, we also indicate whether differences in impacts across subgroups are statistically significant.

Policymakers are likely to be more interested in the effects of Job Corps for program participants than for eligible applicants. However, we present findings for eligible applicants in addition to those for program participants, for two main reasons. First, random assignment was performed at the point that applicants were determined to be eligible for the program; hence, the average characteristics of eligible applicants in the program and control groups were equivalent at random assignment. Therefore, impact estimates per eligible applicant are pure experimental estimates. Impacts per participant, however, were obtained from the impact estimates per eligible applicant under the assumption that the program has no effect on no-shows. While this assumption

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[22] (...continued)
Schochet (2001) discusses how this unobserved mean for program group compliers was computed using observed sample means.

is reasonable, it is difficult to test. Thus, we cannot place as much confidence in these estimates as we can in the impact estimates per eligible applicant.

Second, an important objective of the analysis is to understand the counterfactual for the study by examining the experiences of control group members. When we use the entire control group, this analysis is straightforward, because we can observe their outcomes. Furthermore, we can be confident that these outcomes represent the true counterfactual for the full program group. This analysis is more complicated, however, if we focus on program participants only, because we cannot directly observe the outcomes of those in the control group who would have enrolled in Job Corps had they been given the chance. The average outcomes of these control group members can be estimated as the difference between the average outcomes of program group members who enrolled in Job Corps and the impact estimates per participant. However, these estimated control group means are based on assumptions about the effects of the program on no-shows. Thus, we cannot be sure that they represent the true outcomes of program group enrollees if they had not participated in Job Corps. Consequently, we use the entire control group of eligible applicants to describe the counterfactual for the evaluation, given the importance of this analysis.

### 6. Interpretation of Estimates

The impact analysis generated impact estimates on a large number of outcome measures and for many subgroups. We conducted formal statistical tests to determine whether program and control group differences existed for each outcome measure. However, an important challenge for the evaluation is to interpret the large number of impact estimates to assess whether Job Corps makes a difference and for whom it works.

The initial guide we use to determine whether Job Corps has an impact on a particular outcome measure is the p-value associated with the t-statistic or chi-squared statistic for the null hypothesis of no program impact on that outcome measure. However, we need more stringent criteria than the p-values to identify "true" program impacts, because we are likely to produce significant test statistics by chance (even when impacts may not exist) as a result of the large number of outcomes and subgroups under investigation. For example, in tests of program and control group differences for statistical significance at the 5 percent level, 1 out of 20 independent tests will be significant when in fact no real difference exists.

We also apply three additional criteria to identify potential program impacts:

1. We examine the magnitude of the significant impact estimates to determine whether the differences are large enough to be policy relevant. This is important, as small impacts might be statistically significant because of large sample sizes. For example, for a control group mean of 50 percent, an impact is statistically significant if it is about 2 percentage points or less.

2. We categorize outcomes and subgroups, and look for patterns of significant impacts within and across the categories at each follow-up point and over time. That is, we check that the sign and magnitude of the impact estimates are similar for related outcome measures and subgroups.

3. We determine whether the sign and magnitude of the impact estimates are robust to alternative model specifications and estimation techniques. For example, we conduct sensitivity tests by removing outlier observations, employ different weighting schemes, and estimate impacts using the differences-in-means and regression approaches.

Finally, it is important to recognize that the impact estimates represent the effects of Job Corps for eligible applicants who applied to the program between November 1994 and December 1995. Since most program group members who enrolled in Job Corps were in centers in 1995 and 1996, the estimates may not be representative of the effectiveness of the program as it operates today.

# IV. JOB CORPS EXPERIENCES

Job Corps staff have implemented a well-developed program model throughout the country. Both the model and the fidelity of its implementation are documented in a separate process analysis report (Johnson et al. 1999). For understanding of the impacts that the program may have had on employment and related outcomes of participants, this chapter uses interview data to describe the Job Corps experiences of the program group. Here we note whether program group members received services and then describe the intensity and types of those services.

This chapter answers four broad questions about program participation:

1. Did those who were randomly assigned to the Job Corps program group actually participate?
2. When did most Job Corps participation occur?
3. What were the experiences in the program of those who enrolled?
4. Do the Job Corps experiences of subgroups of interest to the study differ in important ways?

The answers to these questions led to the following conclusions.

First, the program group received extensive Job Corps services. Of those who were assigned to the program group, 73 percent enrolled in Job Corps, 72 percent of these enrollees (just over half the program group) participated in Job Corps for at least three months, and nearly one-quarter of enrollees participated for longer than a year. The average period of participation per enrollee was eight months.

Second, participants enrolled quickly, and most participation occurred during the first 12 months after random assignment. The average participant in the program group enrolled in Job Corps within

1.4 months after random assignment and spent 8 months in the program, which resulted in an average postprogram period of more than three years. Furthermore, the postprogram period was at least two years for about 92 percent of participants. Thus, the 48-month follow-up data provide a reliable indication of the medium-term, postprogram benefits of Job Corps.

Third, enrollees participated extensively in the core Job Corps activities. Most took both academic classes and vocational training, although the relative emphasis differed among individual enrollees. Also, most enrollees participated in the many socialization activities, such as parenting, education, health education, social skills, training, and cultural awareness classes. Many enrollees, however, reported that they did not receive job placement assistance from the program.

Fourth, while many subgroups had different experiences in Job Corps, the differences were small. The mix of academic and vocational training a student received depended on whether the youth had received a high school credential (GED or diploma) before program entry. Students with no credential generally took both academic classes and vocational training. High school graduates spent less time in academic classes and were more likely to focus on vocational training. Nonresidential students (especially females with children) had somewhat lower enrollment rates than residential students. Once in Job Corps, however, the residential and nonresidential students had similar amounts, types, and intensity of training, as well as similar exposure to the other program components. The many other subgroup differences were small, and overall each group's experience was consistent with the conclusions drawn above for the program group as a whole.

The rest of this chapter presents the data supporting these findings. The first section discusses rates and timing of enrollment in Job Corps for those assigned to the program group. The second section discusses the academic classroom and vocational training experiences of enrollees. Third, we discuss the enrollees' participation in other Job Corps activities, such as social skills training

(SST) and parenting classes. Finally, we discuss the child care arrangements used by female enrollees with children while they attended Job Corps. Appendix B presents supplementary tables.[1]

The extent, duration, and intensity of participation may have differed for different groups of students. To identify possible differences, we present tabulations for key subgroups defined by gender and parental status (males, females, and females with children) and for three groups defined by age (16 and 17 years old, 18 and 19 years old, and 20 to 24 years old). Appendix B presents selected data on the program experiences of other important subgroups.

## A. JOB CORPS PARTICIPATION AMONG ELIGIBLE APPLICANTS IN THE PROGRAM GROUP

### 1. Enrollment Rates

The study's program and control groups were established at the point that each youth had been determined to be eligible for Job Corps. An applicant found eligible was assigned to a specific center, and an OA counselor arranged for transportation. However, between the time that eligibility was established and the time that transportation was arranged, some applicants decided not to enroll. Consequently, not everyone who was assigned to the Job Corps program group actually went to a center.

The overall enrollment rate in Job Corps was 73 percent (Table IV.1). This self-reported enrollment rate is practically identical to that calculated from Job Corps administrative records

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[1] The 12- and 30-month follow-up interviews contain detailed questions on program group members' experiences in Job Corps. These interviews captured over 91 percent of all weeks spent in Job Corps. This information, however, was not collected at the 48-month interview. Thus, we used Job Corps administrative data from SPAMIS to measure additional program participation that occurred between the previous interview and the 48-month interview. SPAMIS, however, does not contain detailed information on Job Corps activities (such as participation in SST classes, academic and vocational courses taken, and child care). Thus, descriptive analyses for these activities were conducted using those in the analysis sample who completed 30-month interviews.

## TABLE IV.1

## ENROLLMENT IN JOB CORPS, TIMING OF ENROLLMENT, AND MONTHS OF PARTICIPATION FOR THE PROGRAM GROUP

(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Enrolled in a Job Corps Center | 73.2 | 75.8 | 69.6 | 64.1 | 78.8 | 70.6 | 67.9 |
| Number of Centers Attended | | | | | | | |
| 0 | 26.8 | 24.3 | 30.4 | 35.9 | 21.2 | 29.4 | 32.2 |
| 1 | 65.8 | 67.8 | 62.9 | 58.9 | 71.2 | 62.6 | 61.4 |
| 2 | 7.0 | 7.5 | 6.3 | 4.8 | 7.4 | 7.6 | 5.8 |
| 3 | 0.4 | 0.4 | 0.3 | 0.4 | 0.2 | 0.4 | 0.6 |
| Months Between Random Assignment and Center Enrollment[a] | | | | | | | |
| Less than 0.5 | 48.7 | 48.0 | 49.9 | 47.5 | 49.2 | 48.1 | 48.8 |
| 0.5 to 1 | 25.8 | 25.6 | 26.2 | 24.3 | 25.7 | 25.9 | 25.8 |
| 1 to 3 | 17.4 | 18.0 | 16.6 | 18.8 | 16.9 | 18.1 | 17.4 |
| 3 to 6 | 3.7 | 4.3 | 2.8 | 2.6 | 3.7 | 3.7 | 3.8 |
| 6 or more | 4.3 | 4.1 | 4.6 | 6.9 | 4.5 | 4.2 | 4.2 |
| (Average months) | 1.4 | 1.4 | 1.4 | 1.7 | 1.5 | 1.4 | 1.3 |
| Months Enrolled[a] | | | | | | | |
| Less than 1 | 9.1 | 9.7 | 8.2 | 8.7 | 8.6 | 10.1 | 8.6 |
| 1 to 3 | 19.2 | 20.2 | 17.7 | 19.3 | 22.2 | 17.9 | 15.6 |
| 3 to 6 | 18.6 | 18.7 | 18.5 | 20.9 | 20.1 | 18.4 | 16.3 |
| 6 to 9 | 16.6 | 16.1 | 17.4 | 18.1 | 15.9 | 17.6 | 16.5 |
| 9 to 12 | 12.9 | 12.7 | 13.2 | 12.3 | 11.6 | 13.3 | 14.7 |
| 12 to 18 | 14.4 | 13.5 | 15.9 | 14.1 | 14.3 | 13.8 | 15.5 |
| 18 or more | 9.1 | 9.1 | 9.1 | 6.7 | 7.2 | 9.0 | 12.7 |
| (Average months) | 8.0 | 7.8 | 8.4 | 7.6 | 7.4 | 7.9 | 9.2 |
| Months Between Date Left Job Corps and 48 Months After Random Assignment[a] | | | | | | | |
| Less than 12 | 2.5 | 2.5 | 2.6 | 1.6 | 2.9 | 2.4 | 2.0 |
| 12 to 24 | 5.8 | 5.7 | 6 | 6.6 | 4.9 | 6.4 | 6.8 |
| 24 to 36 | 25.1 | 24.9 | 25.4 | 24 | 24.4 | 23 | 28.9 |
| 36 to 48 | 66.5 | 66.9 | 65.9 | 67.8 | 67.8 | 68.2 | 62.3 |
| (Average months) | 37.5 | 37.7 | 37.2 | 37.7 | 37.7 | 37.7 | 36.9 |
| Enrolled at 48 Months After Random Assignment | 0.3 | 0.3 | 0.4 | 0.3 | 0.4 | 0.3 | 0.1 |
| **Sample Size** | **6,828** | **3,741** | **3,087** | **1,005** | **2,742** | **2,175** | **1,911** |

SOURCE: 12-, 30-, and 48-month follow-up interview and SPAMIS data for those who completed 48-month interviews.

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

[a] Data pertain to program group members who enrolled in a Job Corps center during the 48 months after random assignment.

(Johnson et al. 2000). Most students (90 percent) attended just one center, although 10 percent transferred to another center for regular or advanced training.

Enrollment rates over the 48-month follow-up period differed by subgroup (Table IV.1). Somewhat larger percentages of younger applicants than older applicants enrolled (79 percent compared to 68 percent), and larger percentages of males enrolled than females (76 percent compared to 70 percent). Female applicants with children at baseline had the lowest enrollment rate (64 percent). Rates of participation were somewhat lower for students who were identified at intake as likely nonresidential students than for residential students, 66 percent compared to 75 percent (Table B.5). Furthermore, this relationship between rates of participation for residential and nonresidential students is observed for males, females, and females with children in each residential group.

### 2. Timing of Job Corps Participation

Two aspects of the timing of Job Corps participation are important for the interpretation of program impacts. First, it is useful to know *how long* participants spent in the program, because this is an important measure of *exposure* to the program and of the extent to which program group members invested in their future earning capacity. On the other hand, time spent in the program is time when students probably would have worked, and thus they earned less than they would have if they had not participated.

Second, it is important to know *when participation ended* in order to interpret the impacts on employment, earnings, and related outcomes. One hypothesis of this study is that, for key outcomes like employment and earnings, negative impacts during the in-program period will be offset by positive impacts in the postprogram period. Because Job Corps uses "open-entry" and "open-exit" instruction, the length of participation varies for each student, and no fixed "in-program" period can be identified for all students. Furthermore, waiting times until youths enrolled differed across centers. Thus, impacts defined over a specific time during the 48-month follow-up period are based on some program group members who were still enrolled in Job Corps, some who had been out of Job Corps for a short time, and some who had been out for a longer time. Data on the timing of participation help us identify "in-program" and "postprogram" periods and underscore the need for caution when interpreting impacts over 48 months.

Program group members typically enrolled in Job Corps soon after random assignment (Table IV.1). The average enrollee waited 1.4 months, or just over six weeks, to be enrolled in a Job Corps center, although nearly three-quarters of those who enrolled did so in the first month, and only four percent enrolled more than six months after random assignment.[2]

Once in Job Corps, enrollees participated for about eight months on average, although the period of participation varied considerably (Table IV.1). About 28 percent of all enrollees participated less than three months, and nearly a quarter participated for over a year. Differences across subgroups in average enrollment rates, duration of participation, and length of the follow-up period were generally quite small (Tables IV.1, B.5, and B.6).

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[2] This statistic and all others in the rest of this chapter, except where noted, refer to Job Corps enrollees only. They do not include the 27 percent of program group members who never enrolled in the program.

Variations in the duration of participation in Job Corps resulted in some differences across participants in how much of the 48-month follow-up period was actually a postprogram period. However, most participants had been out of Job Corps for some time at the 48-month point. The average postprogram period for enrollees was 38 months (Table IV.1).[3] In addition, almost 67 percent of enrollees were out of Job Corps for more than three years, and nearly 92 percent were out for more than two years. Less than three percent of enrollees were out for less than one year. Thus, the 48-month employment and earnings results described in Chapter VI should be interpreted as medium-term impacts.

Rates of participation by quarter reveal patterns of participation over time that are useful for interpreting the impact findings. Figure IV.1 shows the fraction of program group members (including the no-shows) who participated in Job Corps during each quarter, measured as 13-week intervals starting from each sample member's date of random assignment.[4] (Table B.1 shows data by gender and age.) The participation rate declined from a peak of 67 percent in the first quarter after random assignment to 21 percent in the fifth quarter (beginning of the second year) and 3 percent in the tenth quarter. By the end of the 48-month period, almost all participants had left Job Corps. Only 0.3 percent of the program group (0.4 percent of enrollees) were in Job Corps in the final week of the 48-month follow-up period.

Based on these broad patterns of participation, we interpret the period from quarters 1 to 4 (months 1 to 12) as largely an "in-program" period. To be sure, some participants left Job Corps near the beginning of this period, and a few had not yet started their training by the end of it. Yet

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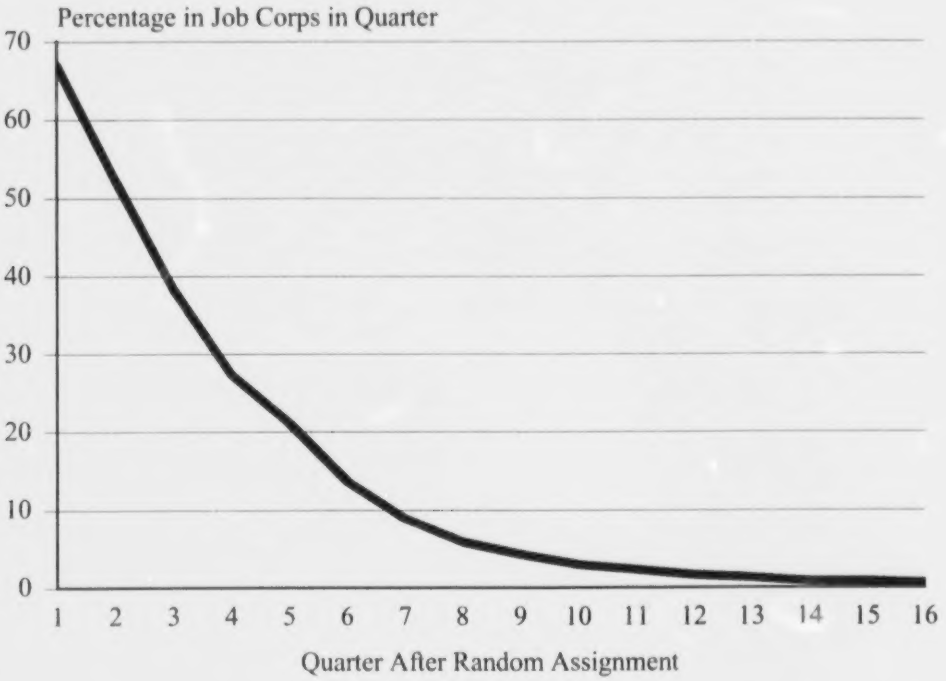
[3] The sum of months before, during, and after Job Corps do not add to 48 months exactly. This is because average length of stay does not include time spent in between spells in Job Corps, for those who left and reentered the program.

[4] Note that here, and throughout the report, quarterly statistics are based on 13-week periods beginning from each enrollee's date of random assignment and thus do not correspond to fixed calendar periods.

FIGURE IV.1

JOB CORPS PARTICIPATION RATES FOR THE FULL PROGRAM GROUP, BY QUARTER



Source: 12-, 30-, and 48-month follow-up interview data and SPAMIS data for those who completed 48-month interviews.

on average just less than half the sample were participating in each quarter. The period from quarters 5 to 8 (months 13 to 24) was a one of transition, in which smaller yet still substantial fractions of the program group were engaged in Job Corps training. The final two years were a postprogram period for most students, although, as noted, a small minority continued to participate in Job Corps. The use of these in-program, transition, and postprogram periods provides a framework for understanding the time profiles of employment and earnings and related impacts.

## B. PARTICIPATION IN JOB CORPS ACADEMIC INSTRUCTION AND VOCATIONAL TRAINING

As the program design intends, a large majority of Job Corps participants (77 percent) took both academic classes and vocational training (Table IV.2). Overall, more than 82 percent of enrollees reported taking academic classes, and nearly 89 percent received vocational training. These patterns are similar for males and females and for younger and older students. The average enrollee reported receiving 1,140 hours of academic and vocational instruction. The average number of weeks that an enrollee participated in academic classes or vocational training (or both) was about 31. A typical high school student receives approximately 1,080 hours of instruction during a school year. Thus, Job Corps provides approximately the equivalent classroom instruction of one year in school.

A few students took only academic classes (5 percent), and a few took only vocational training (12 percent). Most of these were students who participated in Job Corps for a short period, because all students eventually take vocational training and all eventually take a few required academic classes even if they already have a high school credential and solid basic skills. Some students who already had a high school credential and were able to concentrate on vocational training may not have remembered the few academic classes that they took or may not have considered

TABLE IV.2

## COMBINED ACADEMIC AND VOCATIONAL TRAINING PARTICIPATION MEASURES FOR PROGRAM GROUP ENROLLEES

(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Participation in Activity | | | | | | | |
| Took both academic and vocational | 77.3 | 77.8 | 76.5 | 72.1 | 84.2 | 74.6 | 68.4 |
| Took academic classes only | 5.2 | 5.3 | 5.2 | 6.5 | 5.3 | 5.5 | 4.9 |
| Took vocational training only | 11.5 | 11.4 | 11.7 | 13 | 5.3 | 13.5 | 19.9 |
| Took neither | 6.0 | 5.6 | 6.6 | 8.5 | 5.2 | 6.5 | 6.8 |
| Total Hours in Academic Classes and Vocational Training | | | | | | | |
| 0 | 5.9 | 5.7 | 6.3 | 8.2 | 5.3 | 6.4 | 6.5 |
| 1 to 100 | 5.5 | 6.3 | 4.3 | 3.8 | 5.0 | 6.6 | 5.2 |
| 100 to 250 | 10.7 | 11.2 | 10 | 11.6 | 12.9 | 9.4 | 8.5 |
| 250 to 500 | 14.7 | 14.6 | 15 | 16.8 | 14.9 | 15 | 14.1 |
| 500 to 1,000 | 19.9 | 19.5 | 20.5 | 20.1 | 20.7 | 20.4 | 17.7 |
| More than 1,000 | 43.2 | 42.7 | 44.0 | 39.4 | 41.2 | 42.1 | 48.0 |
| (Average hours) | 1,140.0 | 1,130.6 | 1,154.3 | 1,009.9 | 1,093.7 | 1,102.2 | 1,267.6 |
| Number of Weeks Took Academic Classes or Vocational Training | | | | | | | |
| 0 | 5.9 | 5.7 | 6.3 | 8.2 | 5.3 | 6.4 | 6.5 |
| 4 or less | 7.2 | 8.3 | 5.6 | 4.6 | 7.3 | 8.1 | 5.9 |
| 5 to 13 | 20.5 | 21.1 | 19.6 | 23.1 | 23.1 | 19.4 | 17.1 |
| 13 to 26 | 19.3 | 19.0 | 19.8 | 18.9 | 20.0 | 19.1 | 18.5 |
| 26 to 39 | 16.9 | 15.8 | 18.5 | 19.5 | 16.5 | 17.9 | 16.2 |
| 39 to 52 | 12.1 | 12.0 | 12.4 | 11.0 | 11.1 | 12.4 | 13.6 |
| 52 to 78 | 11.8 | 11.9 | 11.7 | 10.3 | 11.6 | 10.5 | 13.7 |
| More than 78 | 6.2 | 6.3 | 6.2 | 4.4 | 5.0 | 6.2 | 8.5 |
| (Average weeks) | 30.5 | 30.1 | 31.0 | 28.2 | 28.9 | 29.8 | 34.0 |
| **Sample Size** | **4,925** | **2,799** | **2,126** | **637** | **2,132** | **1,518** | **1,275** |

SOURCE: 12-, 30-, and 48-month follow-up interview and SPAMIS data for those who completed 48-month interviews.

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

these to be academic classes.[5] A small fraction (six percent) did not participate in either academic or vocational training. These were students who left Job Corps before the end of orientation, which typically lasts two weeks.[6]

Job Corps enrollees received a substantial amount of academic instruction, averaging over 440 hours over 20 weeks (Table IV.3). Mathematics was the most common subject taken: 61 percent of all students said they took it. Just under half reported taking reading. Just over half of all students took GED or high school classes. Most other subjects asked about were reported by 14 to 26 percent of all students. Just three percent of students said they took ESL instruction.

A somewhat higher proportion of students reported taking vocational training (nearly 90 percent, Table IV.4) than reported taking academic instruction (82 percent, Table IV.3). Students also spent on average nearly 28 weeks in vocational training and received 700 hours of vocational instruction. The great amount of time spent in vocational training is consistent with Job Corps's practice of allowing students who enter with a high school credential and good basic skills to focus on vocational training while taking a few required academic classes (for example, health education, parenting, world of work).

Job Corps participants studied a variety of trades. The most popular categories were clerical and construction-related (about 22 percent each), followed by health (15 percent), food service (11 percent), welding (7 percent), and auto mechanics and repair (8 percent).

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[5] Among students who reported only academic classes, nearly 30 percent reported participating in Job Corps for less than one month, and another 45 percent participated for one to three months. Among students who reported taking only vocational training, the distribution of length of stay was more like that for those who took both academic classes and vocational training.

[6] Nearly three-fourths of enrollees who reported taking neither vocational training nor academic classes were enrolled in Job Corps for less than one month.

## TABLE IV.3

## ACADEMIC EXPERIENCES IN JOB CORPS FOR PROGRAM GROUP ENROLLEES

(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Took Academic Classes | 82.3 | 82.8 | 81.6 | 78.5 | 89.1 | 79.9 | 73.3 |
| Total Hours in Academic Classes | | | | | | | |
| 0 | 16.9 | 16.5 | 17.6 | 20.9 | 10.4 | 19.3 | 25.5 |
| 0 to 100 | 15.2 | 16.3 | 13.6 | 13.7 | 14.9 | 16.5 | 14.1 |
| 100 to 250 | 19.2 | 19.5 | 18.7 | 22.2 | 20.0 | 19.8 | 17.0 |
| 250 to 500 | 18.5 | 18.1 | 19.1 | 15.3 | 21.0 | 17.3 | 15.6 |
| 500 to 1,000 | 18.1 | 18.4 | 17.6 | 16.2 | 20.6 | 17.3 | 14.4 |
| More than 1,000 | 12.2 | 11.3 | 13.5 | 11.7 | 13.2 | 9.7 | 13.4 |
| (Average hours) | 439.6 | 425.1 | 461.8 | 401.4 | 482.3 | 389.4 | 426.0 |
| Number of Weeks Took Academic Classes | | | | | | | |
| 0 | 17.2 | 16.8 | 17.7 | 20.9 | 10.4 | 19.6 | 26.2 |
| 4 or less | 10.1 | 10.9 | 8.8 | 7.6 | 9.7 | 11.5 | 9.0 |
| 5 to 13 | 24.7 | 25.0 | 24.3 | 28.3 | 27.0 | 24.3 | 21.2 |
| 13 to 26 | 19.6 | 19.2 | 20.3 | 19.2 | 21.6 | 18.7 | 17.3 |
| 26 to 39 | 12.2 | 11.6 | 13.1 | 10.8 | 13.4 | 12.8 | 9.4 |
| 39 to 52 | 7.1 | 7.2 | 6.8 | 5.3 | 8.3 | 5.4 | 6.9 |
| 52 to 78 | 6.3 | 6.4 | 6.2 | 6.3 | 6.9 | 5.4 | 6.5 |
| More than 78 | 2.8 | 2.9 | 2.8 | 1.5 | 2.7 | 2.4 | 3.6 |
| (Average weeks) | 20.0 | 20.0 | 20.1 | 17.7 | 21.9 | 18.1 | 19.1 |
| Academic Subjects Taken | | | | | | | |
| Reading | 45.8 | 46.7 | 44.4 | 41.8 | 51.9 | 42.1 | 39.6 |
| Writing | 26.2 | 26.0 | 26.5 | 22.8 | 27.2 | 24.5 | 26.5 |
| English language skills | 23.1 | 25.8 | 19.2 | 18.2 | 27.0 | 20.7 | 19.4 |
| ESL | 3.3 | 3.2 | 3.5 | 1.4 | 2.0 | 2.5 | 6.7 |
| GED | 48.1 | 49.6 | 46.0 | 44.8 | 58.7 | 46.1 | 32.1 |
| High school | 3.5 | 3.6 | 3.3 | 2.4 | 4.1 | 3.2 | 2.9 |
| Mathematics | 61.4 | 62.3 | 60.2 | 57.4 | 66.3 | 59.2 | 55.6 |
| Science | 13.6 | 15.5 | 10.8 | 7.1 | 18.2 | 11.9 | 7.7 |
| Other | 22.6 | 23.9 | 20.5 | 21.6 | 24.5 | 20.2 | 21.9 |
| **Sample Size** | **4,925** | **2,799** | **2,126** | **637** | **2,132** | **1,518** | **1,275** |

SOURCE: 12-, 30-, and 48-month follow-up interview and SPAMIS data for those who completed 48-month interviews.

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

## TABLE IV.4

## VOCATIONAL TRAINING EXPERIENCES IN JOB CORPS FOR PROGRAM GROUP ENROLLEES
(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Took Vocational Training | 88.4 | 88.6 | 87.9 | 84.9 | 89.1 | 87.7 | 87.9 |
| Total Hours in Vocational Training | | | | | | | |
| 0 | 11.0 | 10.7 | 11.4 | 14.8 | 10.5 | 11.4 | 11.2 |
| 1 to 100 | 11.1 | 11.1 | 11.0 | 10.4 | 12.6 | 11.4 | 7.8 |
| 100 to 250 | 14.1 | 14.4 | 13.6 | 15.2 | 16.2 | 12.6 | 12.1 |
| 250 to 500 | 16.4 | 16.3 | 16.4 | 16.5 | 17.0 | 16.9 | 14.6 |
| 500 to 1,000 | 21.6 | 21.2 | 22.3 | 21.8 | 21.9 | 20.8 | 22.1 |
| More than 1,000 | 25.9 | 26.2 | 25.5 | 21.4 | 21.7 | 26.9 | 32.2 |
| (Average hours) | 700.4 | 705.5 | 692.5 | 608.4 | 611.4 | 712.8 | 841.6 |
| Number of Weeks Took Vocational Training | | | | | | | |
| 0 | 11.0 | 10.7 | 11.4 | 14.8 | 10.5 | 11.4 | 11.2 |
| 4 or less | 6.6 | 7.3 | 5.7 | 4.2 | 6.8 | 7.6 | 5.1 |
| 5 to 13 | 19.4 | 19.9 | 18.6 | 21.9 | 22.0 | 18.4 | 16.0 |
| 13 to 26 | 19.8 | 19.0 | 21.1 | 20.0 | 20.5 | 19.1 | 19.5 |
| 26 to 39 | 16.8 | 16.1 | 17.9 | 18.6 | 16.4 | 17.8 | 16.3 |
| 39 to 52 | 11.1 | 11.2 | 11.0 | 9.4 | 9.7 | 11.6 | 13.2 |
| 52 to 78 | 10.6 | 11.1 | 9.8 | 7.6 | 10.4 | 9.6 | 12.1 |
| More than 78 | 4.7 | 4.8 | 4.7 | 3.6 | 3.7 | 4.5 | 6.7 |
| (Average weeks) | 27.5 | 27.7 | 27.3 | 24.4 | 26.0 | 27.1 | 30.8 |
| Vocational Trades Taken | | | | | | | |
| Clerical | 21.8 | 11.5 | 37.0 | 39.2 | 18.1 | 22.9 | 26.7 |
| Health | 15.0 | 5.8 | 28.5 | 28.5 | 14.3 | 14.4 | 16.8 |
| Auto mechanics and repair, heavy equipment operator | 7.5 | 11.0 | 2.2 | 1.5 | 8.8 | 5.6 | 7.4 |
| Welding | 7.1 | 10.1 | 2.6 | 1.7 | 8.2 | 6.0 | 6.4 |
| Electrical | 3.1 | 4.7 | 0.7 | 0.3 | 3.4 | 2.7 | 3.0 |
| Other construction trades | 21.3 | 30.2 | 8.0 | 5.1 | 25.6 | 20.0 | 15.4 |
| Food service | 10.8 | 10.1 | 11.9 | 8.6 | 13.1 | 10.4 | 7.3 |
| Electronics | 0.9 | 1.3 | 0.3 | 0.3 | 0.6 | 1.2 | 1.0 |
| Other | 21.7 | 25.1 | 16.7 | 13.9 | 20.4 | 23.3 | 22.2 |
| Schedule of Classes | | | | | | | |
| Every week | 56.5 | 51.2 | 64.5 | 64.8 | 48.5 | 60.1 | 66.3 |
| Alternate weeks | 41.7 | 46.9 | 33.9 | 34.2 | 50.3 | 38.0 | 31.2 |
| Other | 1.8 | 1.9 | 1.5 | 1.0 | 1.2 | 1.9 | 2.5 |
| **Sample Size** | **4,925** | **2,799** | **2,126** | **637** | **2,132** | **1,518** | **1,275** |

SOURCE: 12-, 30-, and 48-month follow-up interview and SPAMIS data for those who completed 48-month interviews.

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

The most notable difference among subgroups is that the youngest students, nearly all of whom did not possess a high school diploma or GED at enrollment, were more likely than older students to say they took both academic classes and vocational training (Table IV.2). Moreover, the younger students reported more hours of academic classes than older students (482 compared with 389 and 426, Table IV.3) and fewer hours of vocational training (611 compared with 713 and 842, Table IV.4). Patterns similar to those of the younger students are also found for older students who enrolled in Job Corps without already holding a high school credential. These patterns of participation reflect the program's emphasis on improving academic skills and achieving a credential for students who come with poor skills, at the same time providing vocational training. Students who already have a high school credential and good skills are encouraged to concentrate on vocational training (though all must take a few key academic classes).[7] Also noteworthy is that, within each age and gender group, the experiences of students designated for residential slots and those designated for nonresidential slots were very similar (Table B.5).

## C. STUDENTS' EXPERIENCES AND PERCEPTIONS OF SELECTED OTHER ACTIVITIES

In addition to formal academic and vocational instruction, Job Corps offers a broad range of activities that are designed to promote health, life skills, and workplace success. While we did not gather detailed data on all domains of center experience, we did ask survey respondents about their experiences with selected activities beyond the core academic classroom instruction and vocational training.[8] Our primary purpose was to assess whether students participated in these activities and whether they thought the activities were useful. (Table IV.5 describes the activities.) Although we asked about academic classes and vocational training in both Job Corps and other programs, we did not ask about these other activities for programs other than Job Corps.

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[7] See Johnson et al. 1999.

[8] Data on these activities were not collected at the 48-month interview. Thus, results presented in this section pertain to those in the 48-month sample who completed 30-month interviews.

Most enrollees said they participated in most of the key activities we asked about. Figure IV.2 shows participation levels for each activity (Table B.2 shows data by gender and age). Almost 82 percent of enrollees reported having attended Progress/Performance Evaluation Panels (P/PEPs). Three-fourths said they took World of Work (WOW), SST, and health classes. Nearly two-thirds of enrollees reported taking cultural awareness and parenting classes. Just less than half of all enrollees took part in the drug and alcohol programs (AODA).

Job placement services was the one area in which well under half of enrollees said they received services (see also Table B.3). Only 40 percent said Job Corps center staff or placement contractor staff had helped them look for a job. This relatively low percentage is consistent with findings on placement services reported in the process report. Johnson et al. (1999) reported that placement contractor staff resources were spread very thin because placement counselors were supposed to serve all students leaving Job Corps for a period of six months. Placement contract managers estimated that their counselors spent half to three-fourths of their time trying to contact former students, many of whom are very mobile, difficult to find, and not interested in receiving placement assistance services. This left very little time for working directly with former students to help them find jobs.[9]

Of those students who reported receiving job placement assistance, just over 41 percent said they got a job as a result of the help they received (Table B.3). Thus, only about 16 percent of all

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[9] Since the period of the study, Job Corps has changed the requirement to serve all terminees, thereby allowing placement contractors to focus efforts on fewer former students.

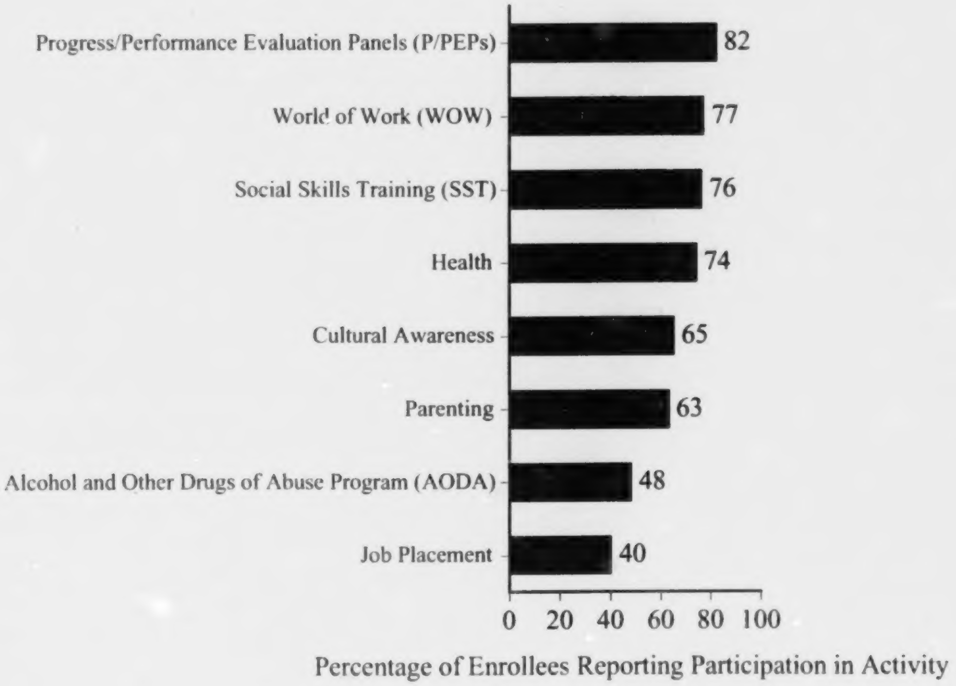
## TABLE IV.5

## DESCRIPTION OF SELECTED JOB CORPS ACTIVITIES

| Activity | Department Providing the Activity | Activity or Topics Covered |
|---|---|---|
| Progress/Performance Evaluation Panels (P/PEP) | Led by the student's counselor, each panel includes a residential living adviser, an education instructor, a vocational instructor, and the student | Meets 30 to 45 days after a student enrolls, and then every 60 days thereafter to review student progress and performance, based on ratings from staff who work with the student |
| World of Work (WOW) | Offered through the academic program | Introductory phase, taught shortly after entry, covers general skills for getting and keeping a job. Exit readiness phase, taught shortly before a student leaves, consists of three units: (1) preparation of a resume, cover letter, and job application; (2) job sources and interviewing; and (3) transition issues |
| Health Education | Offered through the academic department | Units on emotional and social well-being, human sexuality, sexually transmitted diseases, HIV/AIDS, nutrition, fitness, dental hygiene, consumer health, and safety |
| Alcohol and Other Drugs of Abuse Program (AODA) | A unit within Health Education, with specialized counselors | Covers the Job Corps ZT policy, anger control, building self-esteem, and other topics to teach students about decision making. Counselors work with students who test positive for drugs or alcohol upon entry and with others who request help |
| Cultural Awareness | Part of the Intergroup Relations Program offered through the academic department | Topics include living among different cultural groups, acceptance of differences, and discussion of languages, music, food, and art of specific cultural groups |
| Parenting | Offered through the academic department and required for all students | Covers essential parenting skills |
| Social Skills Training (SST) | Offered through the residential living department through small discussion groups led by a residential adviser | Curriculum has 50 lessons, addressing topics like being left out, honesty and accusation, giving and accepting criticism |
| Placement Assistance | Provided by placement assistance contractors | Assist student in finding a job or further education after returning home |

## FIGURE IV.2

## OTHER ACTIVITIES IN JOB CORPS



Source: 12-, 30-, and 48-month follow-up interview data and SPAMIS data for those who completed 48-month interviews.

Note: Questions on these activities in Job Corps were not asked in the 48-month interview. Thus, these figures pertain to those who completed 30-month interviews.

enrollees reported getting a job as a result of placement assistance. This information also appears to be broadly consistent with the administrative data information presented in the process report, which indicates that about half of reported "placements" are "self-placements." (Students who found jobs on their own would be recorded as "placed" in the administrative data, although they might not have received help.)

In addition to measuring whether enrollees participated in the selected activities shown in Table IV.5, we asked students for their opinions about the usefulness of each activity (Table B.4). Specifically, the interview asked whether each activity helped "a lot," "a little," or "not at all." While subjective, the measure does show whether students thought the activities were useful.

Of those who participated in each of the socialization activities, most stated that the activity was helpful. Each program activity was reported to have helped "a lot" by 56 to 61 percent of participants and "not at all" by only about 8 to 15 percent of participants. The remaining 26 to 34 percent (depending on the activity) said the program activity helped "a little." Thus, for each activity, between 85 and 92 percent of students said the activity helped a little or a lot.

## D. CHILD CARE UTILIZATION

About 30 percent of female program group members had children where they enrolled in Job Corps. Furthermore, most of these children were very young (about 85 percent were younger than three years old). Consequently, these mothers had to make child care arrangements to enroll in Job Corps. In fact, an eligibility requirement for Job Corps is that program applicants with children must demonstrate that they have an adequate child care plan for the proposed period of enrollment.

It is often difficult for young disadvantaged mothers to find appropriate child care, and child care is often found to be a significant barrier to attaining economic self-sufficiency for young mothers (Ross 1998). Finding suitable child care is especially challenging for residential females, because they need to find a place where their children can live for a substantial period while they participate in the program. Not surprisingly, then, more than one-half of females with children are nonresidents who live at home. Because the recruitment of young mothers for Job Corps hinges on the ability of these mothers to obtain adequate child care, it is of policy interest to examine the child care arrangements used by those who enroll in the program.

In this section, we briefly discuss the child care arrangements used by mothers who enroll in Job Corps. We focus on mothers only, because although 11 percent of males in our sample had children at program application, only about 20 percent of these fathers lived with their children. Thus, only about 2.5 percent of males needed to find child care. The analysis uses information from the 12- and 30-month interviews on the main child care arrangement used by mothers for their youngest child. We present figures separately for the 374 nonresidential designees and the 242 residential designees because the child care needs differed for these two groups.

Not surprisingly, the most common child care arrangement for both residential and nonresidential designees was care by relatives (including the child's father, grandparents, or other relatives; Table IV.6). However, the child care arrangements for nonresidential designees were much more diverse than for residential designees. Among nonresidential designees, nearly one-half of children were cared for by relatives, about 35 percent were cared for in day care centers, and 12 percent were cared for by nonrelatives (about 60 percent of whom were paid). Among residential designees, however, virtually all (more than 85 percent) were cared for by relatives, most of whom were grandparents. Only about 5 percent of residential designees and 3 percent of nonresidential designees used Job Corps care, because child care programs were available only at 19 centers at the time that our sample was enrolled in Job Corps (Johnson et al. 1999).

## TABLE IV.6

## CHILD CARE ARRANGEMENTS USED BY FEMALES WITH CHILDREN WHILE THEY WERE ENROLLED IN JOB CORPS

(Percentages)

| Type of Child Care Arrangement | Nonresidential Designees | Residential Designees | Total |
|---|---|---|---|
| Relative | 48.4 | 86.9 | 67.1 |
| Child's father or stepfather | 7.5 | 14.1 | 10.6 |
| Child's grandparent | 29.4 | 64.1 | 46.1 |
| Other relative | 11.5 | 8.7 | 10.4 |
| Nonrelative | 11.8 | 0.8 | 6.3 |
| Paid | 7.2 | 0.4 | 3.8 |
| Unpaid | 4.6 | 0.4 | 2.5 |
| Day Care Center, Preschool, or Before- or After-School Program | 34.8 | 4.6 | 19.9 |
| Job Corps Child Care | 3.2 | 5.4 | 4.5 |
| Other | 1.9 | 2.5 | 2.1 |
| **Sample Size** | **374** | **242** | **616** |

SOURCE: 12- and 30-month follow-up interview data for females in the program group who completed 30- and 48-month interviews and who had children while enrolled in Job Corps.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. The child care questions were not asked at the 48-month interview. Thus, the figures pertain to female participants in the analysis sample who completed 30-month interviews and who reported using child care while enrolled at Job Corps at the 12- or 30-month interviews.

# V. EDUCATION AND TRAINING

Job Corps provides intensive academic classroom instruction and vocational skills training to increase the productivity, and hence the future earnings, of program participants. Chapter IV showed that the typical Job Corps student stays in the program for an extended period (about eight months on average). Furthermore, Job Corps serves primarily students without a high school credential (about 80 percent of students do not have a GED or high school diploma at program entry). Thus, participation in Job Corps probably increases the amount of education and training that participants receive and increases their educational levels relative to what they would have been otherwise.

This chapter describes the education and training experiences of program and control group members and provides estimates of the impact of Job Corps on key education and training outcomes during the 48 months after random assignment. We examine education and training experiences of the *program group,* both in Job Corps and elsewhere, to provide a complete picture of the services they received. The education and training experiences of the *control group* are the "counterfactual" for the study. Although control group members were not permitted to enroll in Job Corps for three years after random assignment, they could enroll in all other programs available in their communities. The control group's experiences are a benchmark that shows what education and training the program group would have engaged in had Job Corps not been available. The net increase in education and training due to Job Corps depends critically on what education and training the control group received and what education and training the program group received from other sources, as well as on the education and training the program group received in Job Corps.

This chapter addresses three primary questions:

1. What amount and types of education and training would Job Corps participants receive if they did not participate in Job Corps?
2. Do Job Corps participants receive more education and training than they would have received if they had not participated in Job Corps?
3. Does Job Corps influence educational attainment as measured by the receipt of a GED, vocational certificate, or college degree?

We addressed these questions using survey data on the education and training experiences of sample members during the 48-month follow-up period. For the analysis, we used information on dates of enrollment in education and training programs, the types of programs attended, time spent in academic classes and vocational training, degrees received, and the highest grade completed at the interview date. To compare education and training experiences of members of both the program and control groups, we considered Job Corps along with all other programs, such as English as a Second Language (ESL) and Adult Basic Education (ABE) programs, high school, GED programs, vocational and technical schools, and two-year and four-year colleges. The bulk of education and training for program group members who enrolled in Job Corps came from Job Corps itself, but some enrollees and many program group members who did not enroll in the program (that is, the no-shows) received other types of education and training.

Our analysis distinguishes between academic classroom instruction and vocational training. Academic instruction included classes at regular school or college, as well as classes taken in some other setting for the purpose of improving reading, writing, or mathematics skills; obtaining a GED or high school diploma; or learning English as a second language. Vocational training was for a specific job or occupation and might have been taken in any setting.

We analyzed academic classroom instruction and specific vocational training separately, because provision of both components is one hallmark of Job Corps. Thus, fully understanding Job Corps and the counterfactual against which Job Corps is measured requires describing not only the overall time spent in education and training, but also the time spent in its component parts: academic classes and vocational training.

Many control group members received substantial amounts of education and training. Nearly 72 percent participated in an education or training program during the 48 months after random assignment. On average, they received 853 hours of education and training, roughly equivalent to three-quarters of a year of high school. Participation rates were highest in programs that substitute for Job Corps: GED programs (37 percent), high school (32 percent), and vocational, technical, or trade schools (29 percent).

Job Corps substantially increased the education and training that program participants received, despite the activity of the control group. Nearly 93 percent of the program group engaged in some education or training, compared to about 72 percent of the control group (an impact of 21 percentage points per eligible applicant). The average program group member spent nearly twice as many hours in education and training as the average control group member (7.6 hours per week, compared to 4.1 hours per week). In total, the typical program group member received 1,581 hours of education and training, compared to 853 hours for the typical control group member. Over the 48-month period, Job Corps *participants* spent an average of 4.8 hours per week (998 hours in total) more in programs than they would have if they had not enrolled in the program. This impact per participant corresponds to roughly one school year.

The program group also spent significantly more time in academic classes, and even more in vocational training. Program group members spent an average of 3.1 hours per week (645 hours in total) in academic classes, compared to 2.5 hours per week (520 hours) for the control group (an impact of 0.6 hours per week, or 125 hours in total). The program group typically received about three times more vocational training than the control group (3.1 hours per week, compared to 0.9 hours per week).

Job Corps increased the receipt of GED and vocational certificates but had small negative impacts on the attainment of a high school diploma. Among those without a high school credential at random assignment, about 42 percent of program group members (and 46 percent of program group participants) obtained a GED during the 48-month period, as compared to only 27 percent of control group members (an impact of 15 percentage points per eligible applicant). Similarly, about 38 percent of program group members (and 45 percent of Job Corps participants) reported receiving a vocational certificate, compared to about 15 percent of control group members (an impact of 22 percentage points). Among those without a credential at baseline, a slightly higher percentage of control group members obtained a high school diploma (7.5 percent, compared to 5.3 percent of program group members). Although many of the younger control group members attended high school, most of those in high school did not graduate, because they attended for an average of only about nine months.

At 48 months after random assignment, college attendance and completion had not been affected. About 12 percent of each research group attended a two-year college, and about 3 percent attended a four-year college. Less than 2 percent obtained a two- or four-year college degree.

Finally, impacts on education and training were large across all subgroups defined by youth characteristics. However, the pattern of impacts across age groups exhibited some differences. We find no impacts on hours in academic classes for those 16 and 17 at application to Job Corps, because nearly half of all control group members who were 16 and 17 attended academic classes in high school. However, impacts on hours spent in academic classes were large for the older youths, and hours spent in vocational training were large across all age groups.

The rest of the chapter provides details on our findings. The first section presents impact estimates on participation and time spent in education and training programs, and on types of programs attended. This section also discusses impact findings on time spent in academic classes and vocational training. In the second section, we present impacts on educational attainment. Finally, we present impacts for key subgroups. Supplementary tables are included in Appendix C.

## A. IMPACTS ON PARTICIPATION AND TIME SPENT IN EDUCATION AND TRAINING PROGRAMS

This section compares the participation in education and training programs of the full program and control groups during the 48 months after random assignment. We expected that these impacts would be large during the period soon after random assignment, because many program group members were enrolled in Job Corps then. Job Corps might also increase participation during the postprogram period, because Job Corps encourages students to pursue additional training after finishing Job Corps and helps place them in such programs.

### 1. Impacts on Participation in Education and Training Programs

Many control group members participated in education and training programs (Table V.1). Nearly 72 percent of the control group participated in a program at some point during the 48-month follow-up period. More than one-third (and about 47 percent of those in programs) attended more than one program. Interestingly, the control group participation rate declined only slightly over time. It was about 30 percent per quarter during the first five quarters (that is, 15 months) after random assignment and decreased to about 20 percent between quarters 8 and 16. These high participation rates are not surprising, because control group members demonstrated motivation to

## TABLE V.1

## IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Ever Enrolled in a Program During the 48 Months After Random Assignment | 92.5 | 71.7 | 20.8*** | 100.0 | 28.9*** | 40.5 |
| Number of Programs Ever Enrolled in (Percentages) | | | | | | |
| 0 | 7.6 | 27.9 | -20.4***[d] | -0.3 | -28.3***[d] | -101.0 |
| 1 | 42.0 | 38.3 | 3.7 | 41.6 | 5.1 | 13.9 |
| 2 | 33.4 | 24.8 | 8.6 | 37.7 | 11.9 | 46.1 |
| 3 or more | 17.1 | 9.0 | 8.2 | 21.0 | 11.3 | 117.1 |
| Average Number of Programs Ever Enrolled in | 1.6 | 1.2 | 0.5*** | 1.8 | 0.7*** | 55.4 |
| Percentage Enrolled in a Program, by Quarter After Random Assignment | | | | | | |
| 1 | 76.4 | 29.4 | 47.0*** | 95.0 | 65.3*** | 219.8 |
| 2 | 64.7 | 32.3 | 32.5*** | 79.5 | 45.1*** | 131.0 |
| 3 | 54.0 | 32.2 | 21.8*** | 64.4 | 30.2*** | 88.7 |
| 4 | 45.8 | 32.4 | 13.4*** | 52.4 | 18.6*** | 54.9 |
| 5 | 39.6 | 29.6 | 9.9*** | 44.0 | 13.8*** | 45.6 |
| 6 | 31.4 | 25.9 | 5.5*** | 33.6 | 7.6*** | 29.4 |
| 7 | 26.6 | 23.4 | 3.2*** | 27.9 | 4.5*** | 19.1 |
| 8 | 23.9 | 22.0 | 1.8** | 24.3 | 2.5** | 11.7 |
| 9 | 22.5 | 21.5 | 1.1 | 22.4 | 1.5 | 7.0 |
| 10 | 20.7 | 21.3 | -0.6 | 20.3 | -0.9 | -4.0 |
| 11 | 20.9 | 20.6 | 0.4 | 20.5 | 0.5 | 2.5 |
| 12 | 18.8 | 19.2 | -0.5 | 18.1 | -0.7 | -3.6 |
| 13 | 17.3 | 18.4 | -1.1 | 16.4 | -1.5 | -8.4 |
| 14 | 16.4 | 17.8 | -1.4* | 15.6 | -1.9* | -11.0 |
| 15 | 16.5 | 17.8 | -1.3* | 15.9 | -1.9* | -10.5 |
| 16 | 17.2 | 17.1 | 0.1 | 16.5 | 0.2 | 1.0 |
| Percentage Enrolled in a Program at 48 Months | 13.0 | 12.9 | 0.1 | 12.6 | 0.1 | 1.0 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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obtain training by persisting with their Job Corps application to the point of being determined eligible. Thus, it is not surprising that they had the motivation to find other programs.[1,2]

Despite high control group participation rates, Job Corps substantially increased participation rates in education and training programs (Table V.1). Nearly 93 percent of program group members (and all program group members who enrolled in Job Corps) received some education or training during the four-year observation period, compared to 72 percent of control group members--an impact per eligible applicant of 21 percentage points. The impact per participant was 29 percentage points.

Consistent with this finding is that the typical program group member participated in more programs than the typical control group member (1.6 programs as compared to 1.2 programs). Even among those who participated in education and training programs, the program group participated in more programs. For example, among those who attended programs, about 55 percent of program group members enrolled in at least two programs, as compared to 47 percent of control group members. As discussed below, this is because about 60 percent of Job Corps participants enrolled in another education or training program during the 48-month period (including programs attended before and after they enrolled in Job Corps).

Figure V.1 plots quarterly participation rates in education and training programs by research status. The figure shows the percentage of program and control group members who ever

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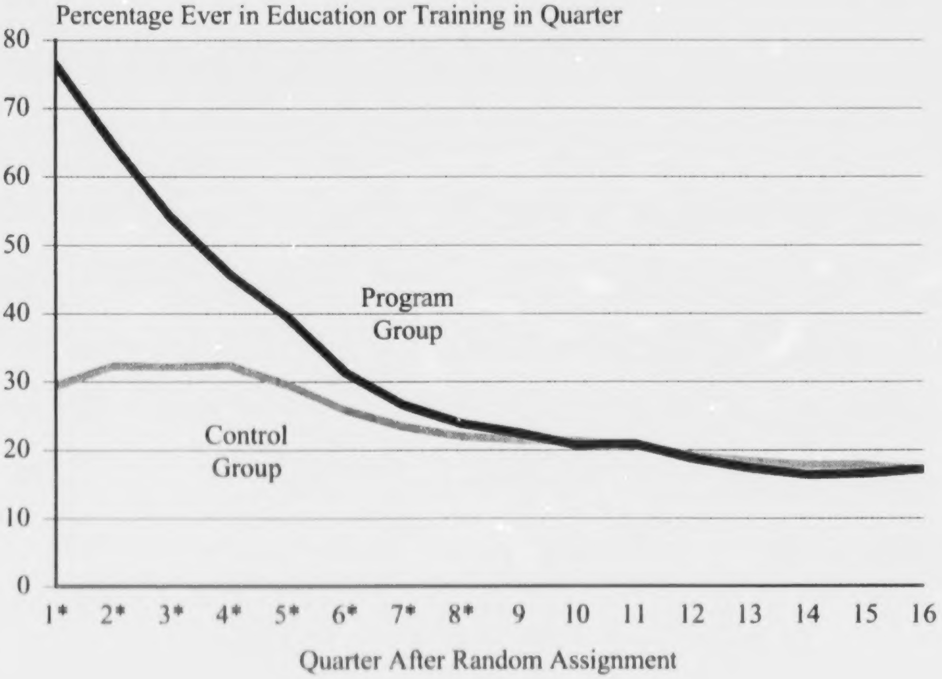
[1] This high rate of attending education and training programs, however, was not due to their exposure to Job Corps. Less than 2 percent of control group members who attended programs before the 12-month interview reported that their most important source of information about the program was the Job Corps OA counselor. Thus, most learned about these programs from other sources (the most common of which were friends, parents, school, and the media).

[2] These educational experiences pertain to eligible program *applicants*, and do not necessarily pertain to the broader population of youths who were eligible for Job Corps but who did not apply to the program.

FIGURE V.1

PARTICIPATION RATES IN EDUCATION AND TRAINING PROGRAMS, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

participated in an education or training program (including Job Corps) during each of the 16 quarters after random assignment. Differences in the program and control group participation rates are estimated impacts per eligible applicant. The statistical significance of these quarterly impacts is denoted by asterisks along the horizontal axis.

The impacts on participation in education-related programs were concentrated in the first six quarters (that is, 18 months) after random assignment. Impacts were large during this period, because many program group members were enrolled in Job Corps then. The quarterly impacts, however, decreased as program group members started leaving Job Corps, and these impacts were not statistically significant at the 5 percent significance level after quarter 8 (that is, after year 2). The impact per eligible applicant was 47 percentage points in quarter 1 and decreased to 22 percentage points in quarter 3 and 10 percentage points in quarter 5. The impact was about 3 percentage points in quarter 7 and near zero in quarters 9 to 16, although enrollment rates were slightly higher for control group members during this period. About 13 percent of both research groups were enrolled in a program during the last week of the 48-month follow-up period.

The finding that similar percentages of program and control group members were enrolled in programs during the postprogram period is important, because it suggests that impacts on employment and earnings during the last two years of the 48-month period were not affected by differences in school enrollment rates by research status.

### 2. Impacts on Time Spent in Education and Training Programs

We report two period-specific measures of time spent in education and training programs: (1) proportion of weeks spent in programs, and (2) hours per week spent in programs. The measures were constructed by dividing the total weeks (or hours) spent in programs during the period by the number of weeks in the period. The measures were set to zero for those who did not participate in education or training programs during the period.

Consistent with the participation findings, impacts on time spent in education and training were positive and large (Table V.2). Program group members spent an average of 24 percent of weeks in programs, compared to 18 percent of weeks for control group members (an impact of 6 percentage points per eligible applicant). Similarly, program group members spent nearly twice as many hours in programs (an average of 7.6 hours per week, as compared to an average of 4.1 hours per week for the control group). Over the entire 48-month (208-week) period, program group members received an average of 1,581 hours of education and training, whereas control group members received an average of 853 hours. Job Corps *participants* spent about 4.8 hours per week (998 hours in total) more in programs than they would have if they had not enrolled in Job Corps. This impact per participant corresponds to roughly one school year. The impact on hours was larger proportionately than the impact on weeks, because Job Corps involves more hours per week than most alternative education and training programs.

Not surprisingly, the time profile of the quarterly impacts on hours per week in programs closely resembles that of the impacts on program participation rates (Table V.2 and Figure V.2). Impacts were largest during the period when many program group members were enrolled in Job Corps, and these impacts decreased as they left the program. Impacts were not statistically significant after quarter 10.

### 3. Impacts on the Types of Programs Attended

Control group members were not permitted to enroll in Job Corps for three years after random assignment. However, many did enroll in other education and training programs in their communities. Therefore, Job Corps opportunities offered to eligible applicants probably reduce their

## TABLE V.2

## IMPACTS ON TIME SPENT IN EDUCATION AND TRAINING PROGRAMS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage of Weeks in Education or Training During the 48 Months After Random Assignment (Percentage) | | | | | | |
| 0 | 8.5 | 30.2 | -21.7***[d] | 0.1 | -30.2***[d] | -99.5 |
| 0 to 25 | 52.8 | 42.1 | 10.7 | 55.8 | 14.8 | 36.1 |
| 25 to 50 | 26.1 | 18.3 | 7.8 | 30.4 | 10.8 | 55.1 |
| 50 to 75 | 9.4 | 6.4 | 3.0 | 10.3 | 4.1 | 67.8 |
| 75 to 100 | 3.3 | 3.0 | 0.3 | 3.4 | 0.4 | 14.6 |
| Average Percentage of Weeks Ever in Education or Training | 24.4 | 18.2 | 6.3*** | 27.1 | 8.7*** | 47.4 |
| Hours per Week Ever in Education or Training (Percentage) | | | | | | |
| 0 | 8.6 | 30.4 | -21.8***[d] | 0.2 | -30.3***[d] | -99.4 |
| 0 to 5 | 35.8 | 41.1 | -5.3 | 32.4 | -7.4 | -18.5 |
| 5 to 10 | 26.7 | 15.0 | 11.7 | 32.0 | 16.2 | 103.1 |
| 10 to 15 | 15.5 | 7.7 | 7.9 | 19.3 | 10.9 | 130.9 |
| More than 15 | 13.4 | 5.9 | 7.5 | 16.2 | 10.5 | 184.9 |
| Average Hours per Week Ever in Education or Training | 7.6 | 4.1 | 3.5*** | 8.9 | 4.8*** | 117.0 |
| Average Hours per Week in Education or Training, by Quarter | | | | | | |
| 1 | 20.9 | 5.5 | 15.4*** | 26.9 | 21.4*** | 392.7 |
| 2 | 20.4 | 6.3 | 14.1*** | 26.3 | 19.6*** | 291.4 |
| 3 | 16.2 | 6.4 | 9.9*** | 20.4 | 13.7*** | 204 |
| 4 | 12.1 | 5.9 | 6.2*** | 14.7 | 8.6*** | 138.9 |
| 5 | 9.6 | 5.4 | 4.2*** | 11.3 | 5.8*** | 104.9 |
| 6 | 7.4 | 4.8 | 2.6*** | 8.5 | 3.7*** | 76.4 |
| 7 | 5.8 | 4.3 | 1.6*** | 6.5 | 2.2*** | 50.6 |
| 8 | 5.0 | 3.9 | 1.2*** | 5.4 | 1.6*** | 42.3 |
| 9 | 4.3 | 3.6 | 0.7*** | 4.4 | 0.9*** | 26.9 |
| 10 | 3.7 | 3.3 | 0.5*** | 3.8 | 0.6*** | 19.9 |
| 11 | 3.6 | 3.3 | 0.3 | 3.6 | 0.4 | 11.4 |
| 12 | 3.2 | 3.2 | 0.0 | 3.2 | 0.0 | 1.4 |
| 13 | 2.9 | 3.0 | -0.2 | 2.8 | -0.2 | -7.1 |
| 14 | 2.6 | 2.8 | -0.2 | 2.6 | -0.3 | -10.6 |
| 15 | 2.5 | 2.7 | -0.2 | 2.4 | -0.3 | -10.2 |
| 16 | 2.5 | 2.6 | -0.1 | 2.5 | -0.1 | -4.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30- and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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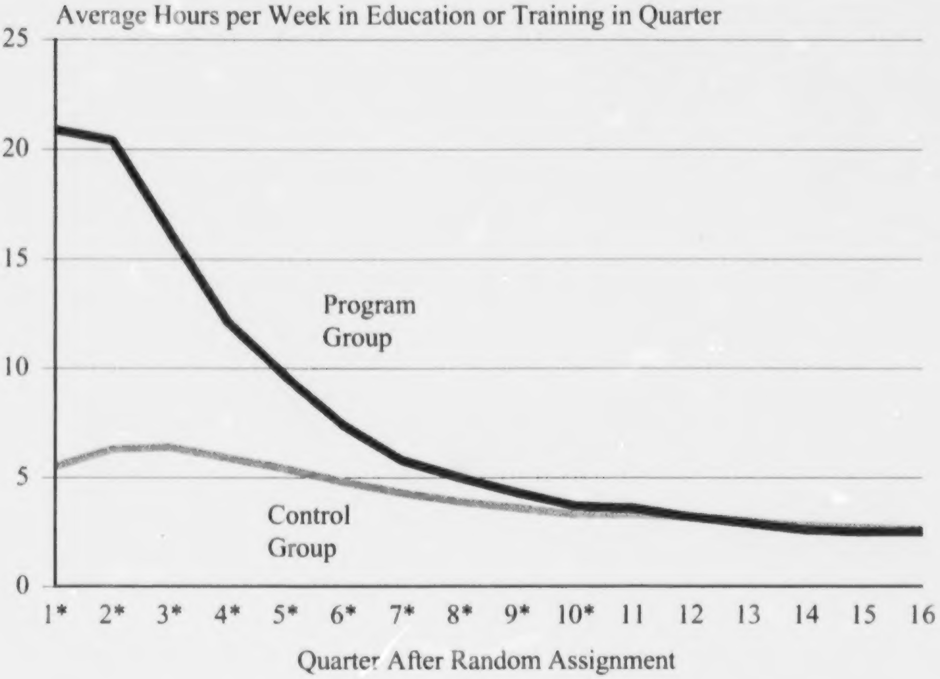
## FIGURE V.2

## AVERAGE HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

participation in other programs that may substitute for Job Corps, such as high school, GED programs, and vocational and technical schools. It is very important to examine impacts on the time spent in these alternative programs, because the net costs of participation in these programs offset the costs of participation in Job Corps in the benefit-cost analysis (McConnell et al. 2001).

Figure V.3 displays data on participation of the program and control groups in several types of education and training programs. Table V.3 provides more details on the calculations.

As noted above, about 71 percent of the control group attended programs other than Job Corps.[3] Participation rates among the control group were highest for programs that could be considered close substitutes for Job Corps: GED programs (42 percent); high school (32 percent); vocational, technical, or trade schools (29 percent); and ESL or ABE classes (9 percent). Only small percentages of the control group attended two-year colleges (12 percent) or four-year colleges (3 percent). Most of those who enrolled in high school or GED programs did so early in the follow-up period (that is, within the first two years after random assignment). However, enrollment in vocational, technical, or trade schools and two-year and four-year colleges continued throughout the follow-up period.

As expected, control group members were more likely than program group members to enroll in a program other than Job Corps during the 48-month period (71 percent as compared to 63 percent). The differences in participation rates in high school, GED programs, vocational schools, and ABE and ESL programs are statistically significant. There were no differences in enrollment rates in two- or four-year colleges.

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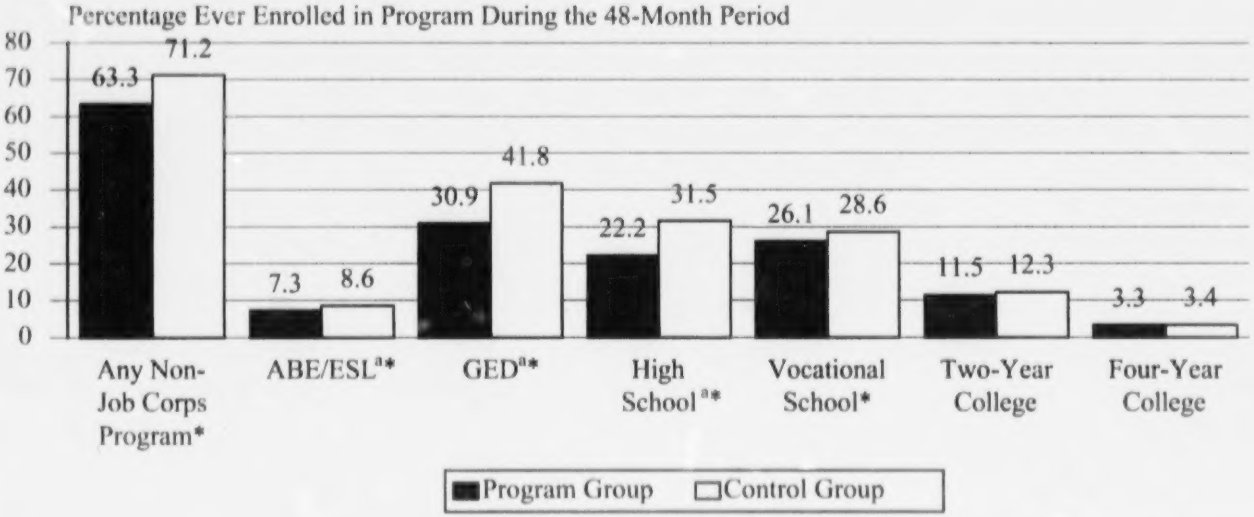
[3] About 4.4 percent enrolled in Job Corps (1.2 percent before their three-year restriction period ended and the remainder afterwards).

FIGURE V.3

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

* Difference between the mean outcome for program and control group members is statistically significan at the 5 percent level. This difference is the estimated impact per eligible applicant.

[a] Figures pertain to those who did not have a high school diploma or GED at random assignment.

## TABLE V.3

## IMPACTS ON PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Types of Programs Ever Attended During the 48 Months After Random Assignment (Percentage) | | | | | | |
| Job Corps | 73.2 | 4.3 | 68.9*** | 100.0 | 95.8*** | |
| Any program other than Job Corps | 63.3 | 71.2 | -7.9*** | 60.2 | -11.0*** | -15.5 |
| ABE or ESL[d] | 7.3 | 8.6 | -1.3** | 6.3 | -1.8** | -21.9 |
| GED[d] | 30.9 | 41.8 | -10.9*** | 26.5 | -15.2*** | -36.4 |
| High school[d] | 22.2 | 31.5 | -9.3*** | 21.6 | -12.9*** | -37.3 |
| Vocational, technical, or trade school | 26.1 | 28.6 | -2.5*** | 24.1 | -3.5*** | -12.7 |
| Two-year college | 11.5 | 12.3 | -0.8 | 11.3 | -1.1 | -9.1 |
| Four-year college | 3.3 | 3.4 | -0.1 | 3.1 | -0.1 | -4.0 |
| Other | 2.8 | 4.0 | -1.2*** | 2.7 | -1.7*** | -38.9 |
| Types of Program Attended During the 24 Months After Random Assignment (Percentage) | | | | | | |
| Job Corps | 72.7 | 1.2 | 71.5*** | 99.3 | 99.3*** | |
| Any program other than Job Corps | 48.9 | 59.7 | -10.8*** | 45.7 | -15.0*** | -24.8 |
| ABE or ESL[d] | 5.1 | 6.3 | -1.2*** | 4.2 | -1.7*** | -29.2 |
| GED[d] | 18.0 | 26.6 | -8.6*** | 15.0 | -11.9*** | -44.3 |
| High school[d] | 18.5 | 26.7 | -8.2*** | 17.9 | -11.4*** | -39.0 |
| Vocational, technical, or trade school | 15.0 | 17.5 | -2.5*** | 13.5 | -3.5*** | -20.4 |
| Two-year college | 7.1 | 7.9 | -0.8 | 6.7 | -1.1 | -14.6 |
| Four-year college | 1.6 | 1.4 | 0.1 | 1.3 | 0.1 | 13.1 |
| Other | 1.4 | 2.0 | -0.6** | 1.3 | -0.8** | -38.4 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] Figures pertain to sample members who did not have a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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Impacts on time spent in alternative education and training programs follow similar patterns (Table C.1). However, the impact on time spent in alternative programs is proportionately larger than the impact on participation rates, because control group members who attended alternative programs did so for longer periods than their program group counterparts (Table C.2). For example, among those who attended high school, control group members were enrolled for an average of 40 weeks (approximately nine months) as compared to an average of 28 weeks for program group members.[4] Among those who enrolled in two-year colleges, the corresponding periods of enrollment were nearly 51 weeks for the control group and 46 weeks for the program group.

While impacts on participation in alternative programs are statistically significant, we were surprised at how small they were. Program group members made considerable use of these same programs, which increased impacts on education and training and reduced the offset to Job Corps program costs. To understand more fully the education and training experiences of the program group outside Job Corps, we tabulated enrollment rates in these programs for Job Corps participants before and after they enrolled in Job Corps, and for the no-shows (Table V.4).

About 15 percent of Job Corps participants attended an education program during the follow-up period *before* they enrolled in Job Corps (that is, between their random assignment and Job Corps enrollment dates). Not surprisingly, most of this activity was high school attendance. This finding is consistent with the fact that about one-quarter of eligible applicants in our sample were in school in the month prior to application to Job Corps (Schochet 1998a), and thus some were still enrolled at random assignment (that is, when they were determined to be eligible for the program).

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[4] These figures were calculated using the results that control group attendees were enrolled for 19.4 percent of weeks during the 208-week period, compared to 13.5 percent of weeks for program group attendees.

## TABLE V.4

## PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS OTHER THAN JOB CORPS FOR JOB CORPS PARTICIPANTS AND NO-SHOWS

(Percentages)

| Programs Ever Attended Other than Job Corps | Job Corps Participants<br>Pre-enrollment | <br>Post-enrollment | No-Shows |
|---|---|---|---|
| Any Program | 15.1 | 49.0 | 71.9 |
| ABE/ESL[a] | 1.7 | 4.6 | 8.5 |
| GED[a] | 2.5 | 23.1 | 37.3 |
| High School[a] | 12.7 | 9.1 | 20.9 |
| Vocational, Technical, or Trade School | 1.7 | 20.6 | 31.5 |
| Two-Year College | 0.3 | 10.1 | 12.1 |
| Four-Year College | 0.0 | 2.8 | 3.7 |
| Other | 0.2 | 2.4 | 3.0 |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

[a] Figures pertain to sample members who did not have a high school credential at random assignment.

About one-half of Job Corps participants enrolled in an education or training program *after* leaving Job Corps.[5] Over 30 percent of Job Corps terminees attended GED programs (23 percent) or returned to high school (9 percent). This group is composed of students who went to Job Corps but did not obtain a high school credential and decided to go back to school in their home community. More than one-third enrolled in vocational or trade schools (21 percent), two-year colleges (10 percent), or four-year colleges (3 percent). While some of these students did not complete Job Corps, this pattern of participation is more consistent with first completing Job Corps and then seeking advanced training after termination.

Finally, many of the 27 percent of program group members who never participated in Job Corps (the no-shows) enrolled in other programs. About 72 percent enrolled in a program during the 48-month period. Interestingly, the pattern of participation in non-Job Corps programs for this group closely follows the pattern for control group members, although high school attendance was somewhat lower.

### 4. Impacts on Participation in Academic Classes and Vocational Training

On the basis of results discussed thus far, we might expect large impacts on time spent in academic classes and vocational training. Job Corps substantially increased time spent in education and training programs during the 48-month period, and most program group Job Corps enrollees participated extensively in the academic and vocational program components.

We also expect larger impacts on the amount of vocational training than on the amount of academic classroom instruction. A large percentage of control group members who attended education and training programs enrolled in high school and GED programs, which are academic programs.[6] A smaller percentage enrolled in vocational programs. Thus, control group members were more likely to receive academic classroom instruction than vocational training, whereas program group members received significant amounts of both. Analysis of impacts on participation in academic instruction and vocational training confirmed these expectations.[7]

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[5] Some youths reported being enrolled in programs outside Job Corps while also enrolled in Job Corps. These programs were excluded from Table V.4.

Program group members received substantially more academic classroom instruction than did control group members (Figure V.4 and Table V.5). About 81 percent of program group members (and 91 percent of Job Corps participants) ever took academic classes during the 48 months after random assignment, as compared to 57 percent of control group members (an impact of 24 percentage points per eligible applicant). Similarly, the impact per eligible applicant on hours per week in academic classes was 0.6 hours (an average of 3.1 hours for the program group and 2.5 hours for the control group). These figures translate to about 645 hours of academic classroom training for the typical program group member over the 48-month period and 520 hours for the typical control group member. Not surprisingly, impacts occurred primarily during the first 12 months after random assignment (the in-program period). Most of the academic instruction received by the program group took place in Job Corps, whereas most of the academic instruction received by the control group took place in high school, GED, and ABE programs (Table C.3).

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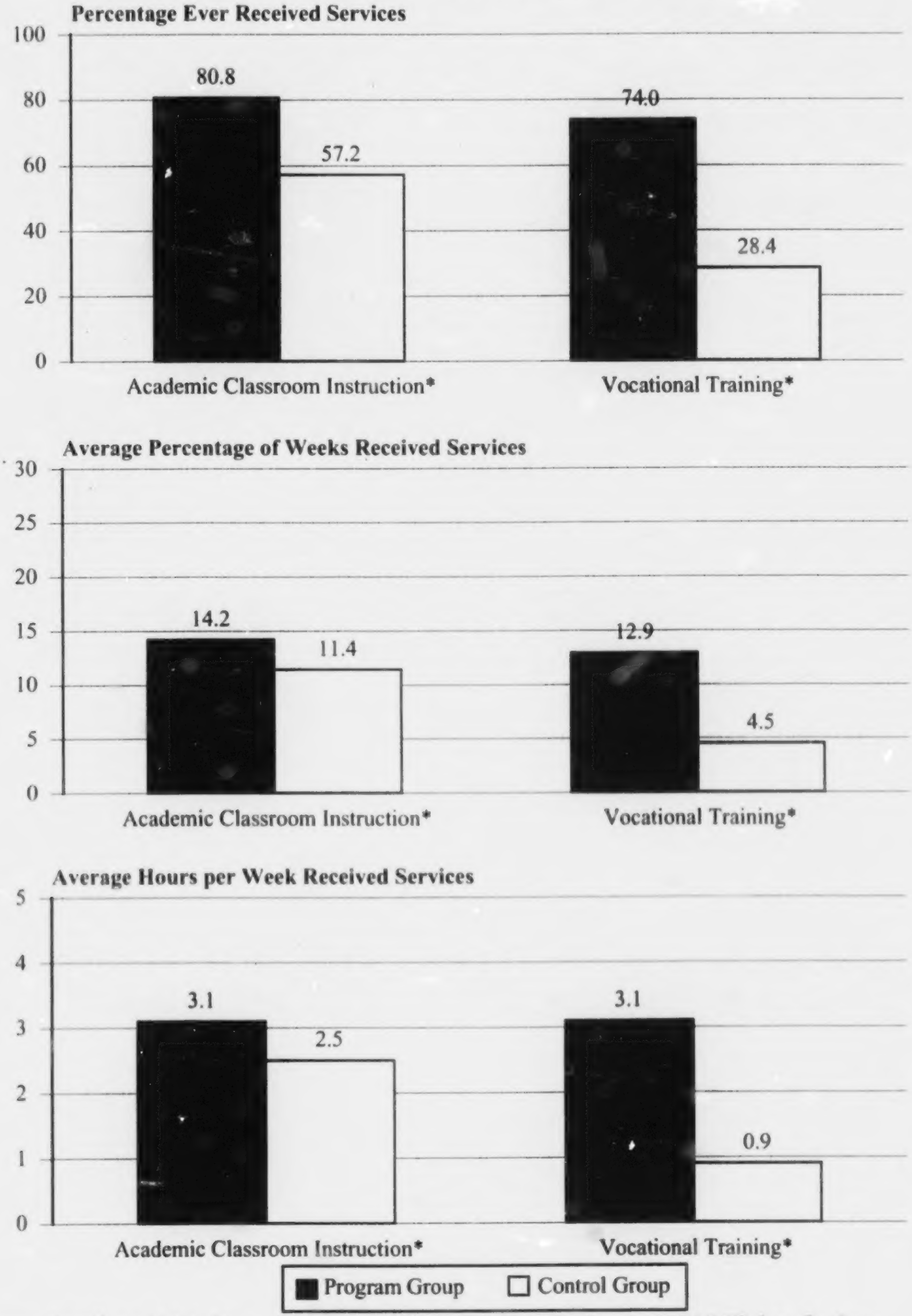
[6] Students who said they were attending a GED course were assumed to be in an academic program. Students who said they were attending high school were asked separately about academic and vocational instruction.

[7] The part of the 30-month follow-up questionnaire that collected information on academic and vocational training was changed in the middle of data collection to correct an error in the instrument's skip logic. Therefore, among those in the 48-month sample who completed 30-month interviews, results on vocational and academic training are based on a restricted sample consisting of those whose 30-month interview took place after April 1998, or about 45 percent of the full 30-month sample. Any differences between those interviewed early and later in the cycle are likely to be equally present, on average, in both program and control groups. The sample for this analysis also includes all those who completed a 48-month interview but not a 30-month interview. Thus, the impact estimates, though probably unbiased, may not be representative of the full sample.

FIGURE V.4

PARTICIPATION IN ACADEMIC CLASSES AND VOCATIONAL TRAINING DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT



Source: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE V.5

## IMPACTS ON PARTICIPATION IN ACADEMIC CLASSES

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Ever Took Academic Classes During the 48 Months After Random Assignment | 80.8 | 57.2 | 23.7*** | 90.6 | 32.9*** | 57.0 |
| Percentage Took Academic Classes, by Quarter After Random Assignment | | | | | | |
| 1 | 64.7 | 26.0 | 38.7*** | 81.0 | 53.7*** | 197.2 |
| 2 | 55.6 | 27.7 | 27.9*** | 69.1 | 38.7*** | 127.4 |
| 3 | 46.5 | 27.8 | 18.7*** | 56.1 | 26.0*** | 86.4 |
| 4 | 39.6 | 27.3 | 12.2*** | 45.7 | 17.0*** | 59.1 |
| 5 | 34.6 | 25.9 | 8.7*** | 39.1 | 12.1*** | 44.7 |
| 6 | 26.5 | 20.8 | 5.7*** | 29.0 | 7.9*** | 37.6 |
| 7 | 21.5 | 18.7 | 2.8*** | 23.0 | 3.9*** | 20.7 |
| 8 | 18.5 | 17.1 | 1.4* | 19.2 | 1.9* | 11.0 |
| 9 | 17.0 | 16.8 | 0.2 | 17.3 | 0.3 | 1.7 |
| 10 | 15.4 | 16.0 | -0.6 | 15.3 | -0.9 | -5.5 |
| 11 | 13.1 | 12.4 | 0.7 | 13.0 | 1.0 | 8.0 |
| 12 | 7.1 | 6.5 | 0.6 | 7.2 | 0.8 | 12.4 |
| 13 | 5.6 | 5.2 | 0.3 | 5.8 | 0.4 | 8.0 |
| 14 | 4.7 | 4.8 | -0.2 | 4.5 | -0.2 | -5.0 |
| 15 | 4.7 | 4.6 | 0.1 | 4.7 | 0.2 | 3.4 |
| 16 | 4.3 | 4.0 | 0.3 | 4.5 | 0.4 | 10.0 |
| Average Percentage of Weeks in Academic Classes, by Year | | | | | | |
| All years | 14.2 | 11.4 | 2.7*** | 15.6 | 3.8*** | 32.6 |
| 1 | 30.3 | 19.4 | 11.0*** | 35.4 | 15.3*** | 75.6 |
| 2 | 16.5 | 16.0 | 0.5 | 17.2 | 0.7 | 4.1 |
| 3 | 8.7 | 8.7 | 0.0 | 8.4 | 0.0 | -0.2 |
| 4 | 3.2 | 3.5 | -0.3 | 3.0 | -0.4 | -12.1 |
| Average Hours per Week in Academic Classes, by Year | | | | | | |
| All years | 3.1 | 2.5 | 0.6*** | 3.4 | 0.8*** | 31.2 |
| 1 | 6.8 | 4.9 | 1.9*** | 7.9 | 2.7*** | 51.2 |
| 2 | 3.4 | 3.2 | 0.2 | 3.6 | 0.3 | 9.8 |
| 3 | 1.6 | 1.6 | 0.1 | 1.6 | 0.1 | 6.5 |
| 4 | 0.5 | 0.6 | -0.1 | 0.5 | -0.1 | -14.5 |
| **Sample Size** | **3,378** | **2,346** | **5,724** | **2,410** | | |

SOURCE: Baseline and 12-, 30- and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job

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Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

Impacts on the amount of vocational training were larger (Figure V.4 and Table V.6). The percentage of program group members who received vocational training was nearly three times that for the control group (74 percent as compared to 28 percent). Furthermore, average hours per week in vocational training was more than three times higher for the program group (3.1 hours per week, compared to 0.9 hours per week for the control group). Program group members had an average of 645 hours of vocational training over the 48-month period, compared to 187 hours per control group member. Impacts were largest during the first year after random assignment, when many program group members were enrolled in Job Corps, although they were still positive and statistically significant during the second year and even the third year.

## B. IMPACTS ON EDUCATIONAL ATTAINMENT

Job Corps substantially increased the overall time youths devoted to education and training programs, as well as time devoted to academic instruction and vocational training. Did these increases in effort lead to gains in the attainment of GED certificates, vocational certificates, and college degrees or to gains in years of school completed?

Job Corps could affect attainment of a high school credential and a vocational certificate, because of both the additional time devoted to training and the emphasis placed on reaching these milestones. In all Job Corps centers, the academic department emphasizes helping students who do not have a high school credential at program entry to obtain a GED. About one-quarter of centers are also accredited to grant a high school diploma. Reflecting the importance that program managers attach to these goals, the Job Corps performance measurement system incorporates strong incentives promoting it. At the time program group members were enrolled, performance ratings of center operators depended directly on how many students earned a GED or diploma.

## TABLE V.6

## IMPACTS ON PARTICIPATION IN VOCATIONAL TRAINING

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Ever Received Vocational Training During the 48 Months After Random Assignment | 74.0 | 28.4 | 45.6*** | 91.1 | 63.4*** | 229.0 |
| Percentage Received Vocational Training, by Quarter After Random Assignment | | | | | | |
| 1 | 62.2 | 5.5 | 56.7*** | 82.9 | 78.8*** | 1,944.2 |
| 2 | 53.3 | 6.0 | 47.3*** | 71.0 | 65.7*** | 1,246.9 |
| 3 | 41.3 | 5.9 | 35.4*** | 54.6 | 49.2*** | 903.3 |
| 4 | 31.2 | 6.6 | 24.6*** | 40.6 | 34.1*** | 528.5 |
| 5 | 26.5 | 7.0 | 19.5*** | 33.4 | 27.1*** | 429.9 |
| 6 | 18.8 | 6.1 | 12.7*** | 23.1 | 17.6*** | 324.4 |
| 7 | 14.2 | 5.4 | 8.7*** | 16.8 | 12.1*** | 255.0 |
| 8 | 11.4 | 5.4 | 6.0*** | 13.2 | 8.3*** | 170.4 |
| 9 | 9.9 | 5.5 | 4.4*** | 11.1 | 6.2*** | 125.4 |
| 10 | 8.7 | 5.9 | 2.9*** | 9.4 | 4.0*** | 73.3 |
| 11 | 8.5 | 6.0 | 2.5*** | 9.0 | 3.4*** | 62.4 |
| 12 | 7.2 | 5.8 | 1.4*** | 7.6 | 1.9*** | 34.4 |
| 13 | 6.5 | 5.9 | 0.5 | 6.6 | 0.7 | 12.6 |
| 14 | 6.5 | 6.1 | 0.4 | 6.2 | 0.5 | 8.6 |
| 15 | 6.4 | 6.0 | 0.5 | 6.2 | 0.7 | 12.5 |
| 16 | 6.4 | 6.2 | 0.2 | 6.0 | 0.3 | 5.3 |
| Average Percentage of Weeks Received Vocational Training, by Year | | | | | | |
| All years | 12.9 | 4.5 | 8.5*** | 16.1 | 11.8*** | 273.5 |
| 1 | 30.1 | 5.1 | 25.0*** | 39.6 | 34.7*** | 712.4 |
| 2 | 11.8 | 4.6 | 7.2*** | 14.4 | 10.1*** | 230.4 |
| 3 | 5.8 | 4.1 | 1.7*** | 6.3 | 2.3*** | 58.5 |
| 4 | 4.5 | 4.0 | 0.5 | 4.4 | 0.7 | 17.8 |
| Average Hours per Week Received Vocational Training, by Year | | | | | | |
| All years | 3.1 | 0.9 | 2.2*** | 3.9 | 3.1*** | 355.4 |
| 1 | 7.3 | 1.0 | 6.4*** | 9.7 | 8.8*** | 1,019.1 |
| 2 | 2.9 | 1.0 | 1.8*** | 3.5 | 2.5*** | 265.4 |
| 3 | 1.3 | 0.9 | 0.4*** | 1.5 | 0.6*** | 67.3 |
| 4 | 1.0 | 0.8 | 0.2 | 1.0 | 0.2 | 32.7 |
| **Sample Size** | **3,378** | **2,346** | **5,724** | **2,410** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

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TABLE V.6 *(continued)*

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[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

A defining feature of the Job Corps vocational education program is its emphasis on competency-based instruction. Each trade follows a prescribed plan of activities and has criterion-referenced measurements that are used to verify student competencies in each of the skills required of an entry-level position in an occupation. Students receive vocational certificates at various step-off levels. Currently, performance ratings depend on ensuring that students complete Job Corps and secure jobs or postprogram training. Obtaining a GED or completing vocational training are requisites for defining a student as a Job Corps completer.

It is unclear whether Job Corps is likely to affect attainment of a high school diploma. On the one hand, as noted, about one-quarter of Job Corps centers can grant state-recognized high school diplomas. On the other hand, the alternative to Job Corps includes a substantial amount of attendance in high school. Which effect is stronger is an empirical question.

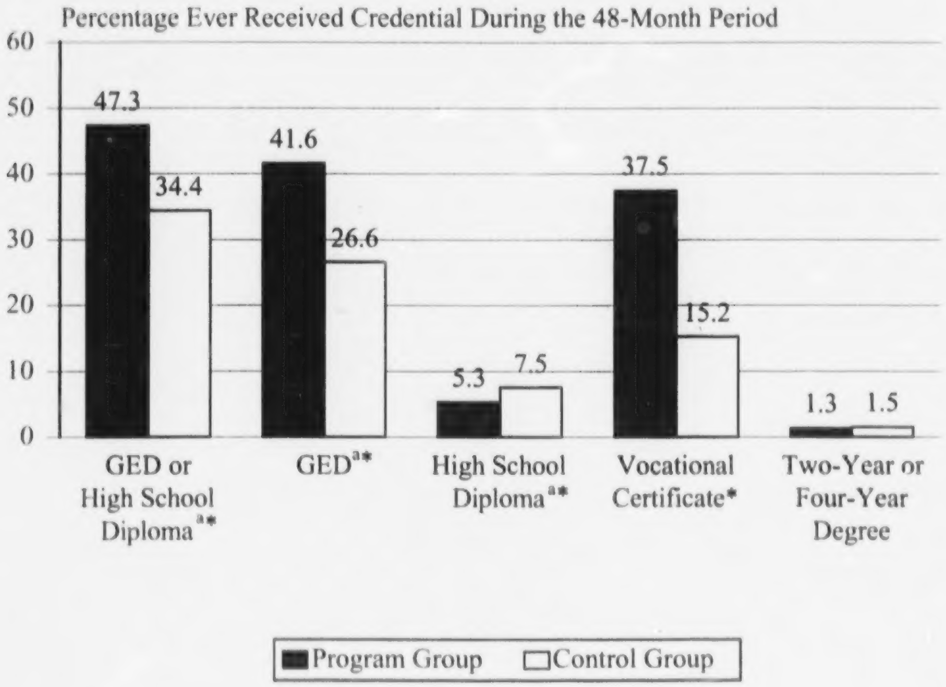
### 1. Impacts on the Attainment of a High School Credential

Job Corps had a large positive impact on GED completion for the 80 percent of youths without a high school credential at random assignment (Figure V.5 and Table V.7). Of those who did not already have a high school credential, 42 percent of the program group and 27 percent of the control group received a GED, an impact of 15 percentage points per eligible applicant. About 46 percent of program group members who enrolled in Job Corps without a credential received a GED.

Few youths without a high school credential at random assignment obtained a high school diploma, although slightly more control group members did so (Figure V.5 and Table V.7). Among those without a credential at baseline, 7.5 percent of control group members obtained a high school diploma, as compared to 5.3 percent of program group members (a statistically significant impact of -2.2 percentage points per eligible applicant). As discussed, about 32 percent of dropouts in the

FIGURE V.5

DEGREES, DIPLOMAS, AND CERTIFICATES RECEIVED



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

[a]Figures pertain to those who did not have a high school credential at random assignment.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE V.7

## IMPACTS ON EDUCATIONAL ATTAINMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Degrees, Diplomas, and Certificates Received During the 48 Months After Random Assignment (Percentage) | | | | | | |
| GED certificate or high school diploma[d] | 47.3 | 34.4 | 12.9*** | 51.4 | 18.0*** | 53.8 |
| GED certificate[d] | 41.6 | 26.6 | 15.0*** | 46.3 | 20.9*** | 82.3 |
| High school diploma[d] | 5.3 | 7.5 | -2.2*** | 4.7 | -3.1*** | -40.1 |
| Vocational, technical, or trade certificate | 37.5 | 15.2 | 22.3*** | 45.1 | 30.9*** | 218.7 |
| College degree (two-year or four-year) | 1.3 | 1.5 | -0.2 | 1.2 | -0.3 | -19.1 |
| Highest Grade Completed at the 48-Month Interview | | | | | | |
| Less than 9 | 6.7 | 5.9 | 0.8 | 7.0 | 1.1 | 18.9 |
| 9 to 11 | 58.9 | 59.5 | -0.5 | 60.2 | -0.7 | -1.2 |
| 12 | 27.5 | 27.6 | 0.0 | 26.7 | 0.0 | -0.2 |
| Greater than 12 | 6.8 | 7.1 | -0.2 | 6.1 | -0.3 | -4.9 |
| Average Highest Grade Completed | 10.7 | 10.8 | 0.0 | 10.7 | 0.0 | -0.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] Figures pertain to sample members who did not have a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

control group enrolled in high school. Thus, just 23 percent of those who attended high school obtained a high school diploma. This low completion rate was due to the fact that students in high school attended for an average of only about nine months, while the average dropout had completed less than the 10th grade at the time of Job Corps enrollment.

Overall, program group dropouts were much more likely than control group dropouts to obtain a high school credential (either a GED certificate or a high school diploma) during the 48-month period (47 percent, compared to 34 percent). These impacts were large, because Job Corps slightly reduced the high school diploma completion rate but substantially increased the GED completion rate.

The rate of high school completion for the control group was similar to the rate for low-income dropouts based on data from the 1988 National Education Longitudinal Study (NELS). Among low-income 1988 eighth-graders who dropped out of high school at least once between 1988 and 1992, about 20 percent received a GED by 1994 (as compared to 27 percent of the control group), and about 13 percent obtained a high school diploma by 1994 (as compared to about 8 percent of the control group).[8]

The high school diploma and the GED are both meant to certify completion of a secondary school education. However, some have argued that a GED is worth less than a diploma in the labor market (Heckman and Cameron 1993; and Boesel et al. 1998), although the empirical evidence is mixed. Furthermore, it may be that a GED earned through a special program such as Job Corps is more valuable than one earned, for example, as a result of a narrowly focused test-preparation course. We examine the extent to which earnings impacts differed for those who completed a GED and those who did not in a separate report (Gritz et al. 2001).

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[8] See Berktold et al. 1998.

### 2. Impacts on the Attainment of a Vocational Certificate

Job Corps had very large impacts on the attainment of a vocational certificate (Figure V.5 and Table V.7). The estimated impact was 22 percentage points (38 percent of the program group received a vocational certificate, compared to 15 percent of the control group), and is even larger than the GED impact.

The emphasis given to documenting progress and certifying vocational completion in Job Corps creates a need for caution in interpreting these large impacts. The unique structure of Job Corps may have made program group members more likely to receive a vocational certificate than control group members who achieved similar levels of competency in alternative vocational programs. Still, the impacts on vocational certification are in line with impacts on receipt of vocational training, which lends credence to the findings.

### 3. Impacts on the Attainment of a College Degree

As discussed, only a small percentage of either the control group or the program group attended two-year or four-year colleges during the 48 months after random assignment. Thus, less than 2 percent of youth in both groups earned a two- or four-year college degree (Figure V.5 and Table V.7).

### 4. Impacts on Highest Grade Completed

Because we find few differences by research status in the attainment of high school diplomas or college degrees, it is not surprising that we find no impact on years of formal schooling completed at the 48-month interview (Table V.7). The average highest grade completed was about 10.7 for both groups (as compared to 10.1 for both groups at random assignment), and the distributions of highest grade completed were nearly identical for the two groups. These results reflect the fact that youth who attended formal school did not remain there for very long.

These results suggest that Job Corps does not affect the educational attainment as measured by self-reported grade completion, which presumably includes only formal schooling and thus captures only one dimension of education. Those who participated in GED programs or other academic courses outside a regular high school were not likely to have reported a change in their highest grade completed, nor were those whose training activities were vocational.

Self-reports of highest grade completed are somewhat unreliable. This is evident in the many inconsistent responses given by the same person from one interview to the next, such as "highest" grade levels that went down over time. Indeed, researchers who study educational attainment have noted the presence of measurement error in this kind of report (Ashenfelter and Krueger 1994). We estimated impacts using a number of alternative measures of highest grade completed, including the maximum report and an "edited" version based on alternative rules for eliminating or recoding certain suspicious or inconsistent cases. The particular correction did affect the final attainment levels, but it had no effect on the finding that program and control group differences were negligible.

## C. FINDINGS FOR SUBGROUPS

This section presents data on the education and training experiences of key subgroups defined by youth characteristics at baseline. We focus our discussion on subgroups defined by age at application to Job Corps and high school credential status at random assignment. These subgroups are of particular interest because of substantial differences in their skill levels and educational needs at baseline.

In the rest of this section, we present evidence that for broad groups of youths served by Job Corps, the program had a very large effect on time spent in education and training and on the attainment of a GED (for those without a high school credential at baseline) and vocational certificate. First, we present findings for subgroups defined by age and high school credential status.

We examine the experiences of (1) those 16 and 17, (2) those 18 to 24 who did not have a high school credential, and (3) those 18 to 24 who had a high school credential. Nearly all those in our sample who were 16 and 17 years old did not have a high school credential, compared to 73 percent of those 18 and 19 and 50 percent of those 20 to 24. We combined the 18- and 19-year-old dropouts with the 20- to 24-year-old dropouts, because the education and training experiences and impact findings were very similar for these groups. For similar reasons, we also combined the two older groups with a high school credential. Then, we briefly present findings on key outcomes for other youth subgroups defined by gender, residential designation status, arrest history, race, and ethnicity, and date of application to Job Corps. We present findings using a series of figures and charts. Tables C.4 to C.6 present more details.

### 1. Impacts by Age and High School Credential Status

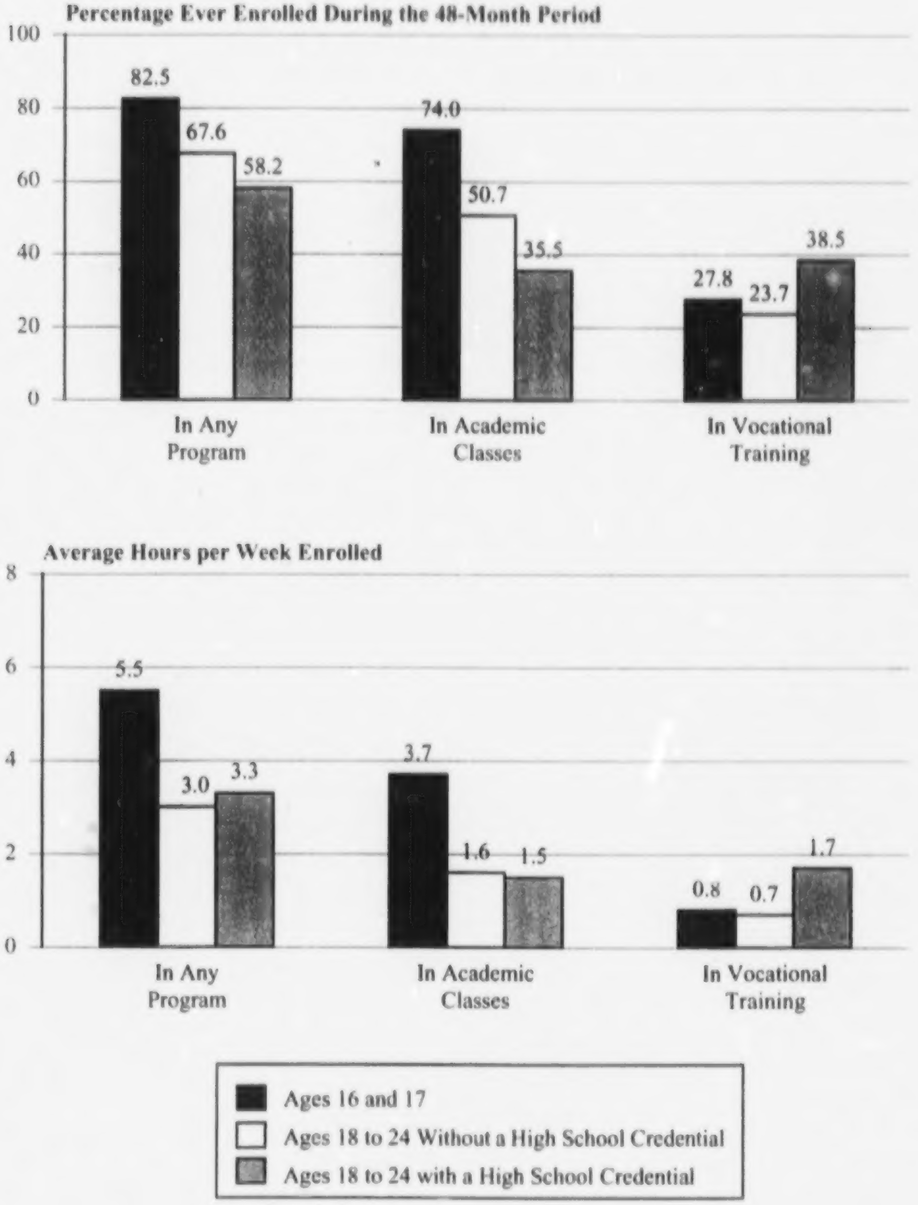
Our impact findings for subgroups defined by age and educational level at baseline were largely due to subgroup differences in the experiences of control group members. Program group experiences varied less because, as discussed in Chapter IV, all subgroups of participants received substantial amounts of education and training in Job Corps. We first discuss the control group experiences, then the impact findings.

#### a. Control Group Experiences

Among the control group, levels of participation in education and training programs were higher for those 16 and 17 than for the older youth (Figure V.6). About 83 percent of those 16 and 17 ever enrolled in a program during the 48-month period, compared to 68 percent of the older youth without a high school credential at baseline and 58 percent of the older graduates. Similarly, the youngest control group members spent an average of 5.5 hours per week (1,144 hours during the 48-month

### FIGURE V.6

## PARTICIPATION AND HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS FOR CONTROL GROUP MEMBERS, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



Source: Baseline, 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

period) in programs, whereas the older groups spent only about 3.2 hours per week in programs (about 666 hours in total).

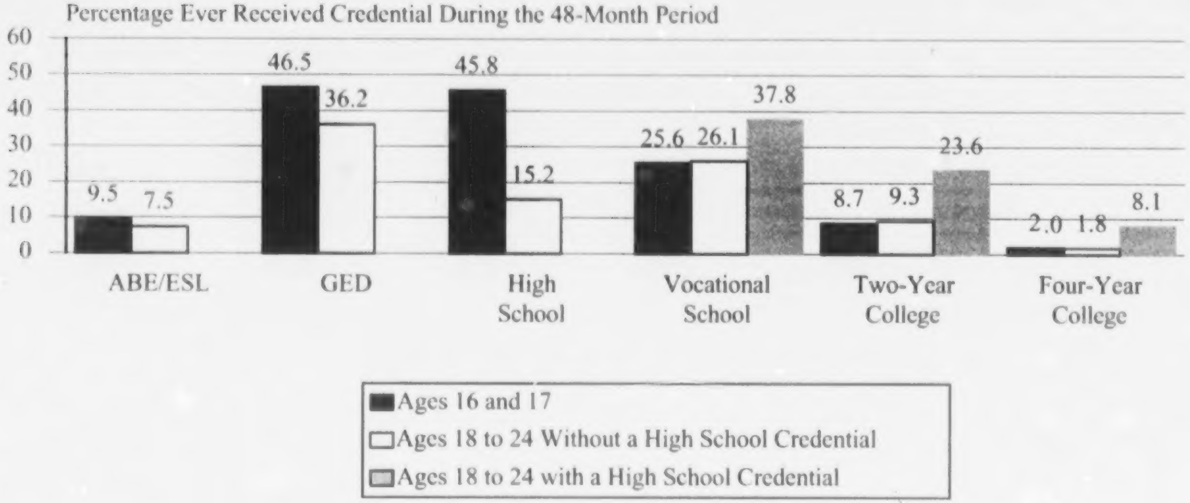
The time profile of participation in programs also differed for the younger and older control group members, although similar percentages were in programs late in the observation period (Tables C.4 to C.6). About 45 percent of the 16- and 17-year-olds were enrolled in programs during each of the first five quarters after random assignment, but the participation rate dipped to about 30 percent in quarter 7 and about 20 percent after quarter 10. The participation rate for the older groups, however, remained constant at about 20 percent per quarter throughout the follow-up period. Importantly, the control group participation rates were about 20 percent for all age groups during the postprogram period, so the earnings impacts by age were not differentially affected by differences in school enrollment rates.

The younger control group members spent more time in programs than the older ones, because they spent much more time in academic classes--but not in vocational training (Figure V.6). The typical 16- and 17-year-old control group member spent 3.7 hours per week in academic classes but only 0.8 hours per week in vocational training (so that more than 80 percent of total hours spent in programs were spent in academic classes). On the other hand, the older high school completers spent more than double the hours in vocational training than the younger group, but spent substantially fewer hours in academic classes.

These findings reflect the types of programs that control group members attended (Figure V.7). Many 16- and 17-year-olds attended academic programs, but fewer went to vocational programs. About half of these youth attended high school, and about half attended GED programs. About one-

FIGURE V.7

PARTICIPATION IN EDUCATION AND TRAINING PROGRAMS FOR CONTROL GROUP MEMBERS, BY TYPE OF PROGRAM, AGE, AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

quarter attended vocational and technical schools, and about 9 percent enrolled in two-year colleges. Because most of the schooling for this group took place in high school and GED programs, it is not surprising that the youngest control group members received large amounts of academic classroom instruction and smaller amounts of vocational training.

In contrast, the older graduates tended to enroll in programs that offer vocational training: nearly 40 percent enrolled in vocational schools, and nearly one-quarter enrolled in two-year colleges. Thus, these youth received more vocational training than their counterparts. Participation rates among the older dropouts were largest in GED programs (about 36 percent) and vocational programs (about 26 percent); only about 15 percent enrolled in high school.

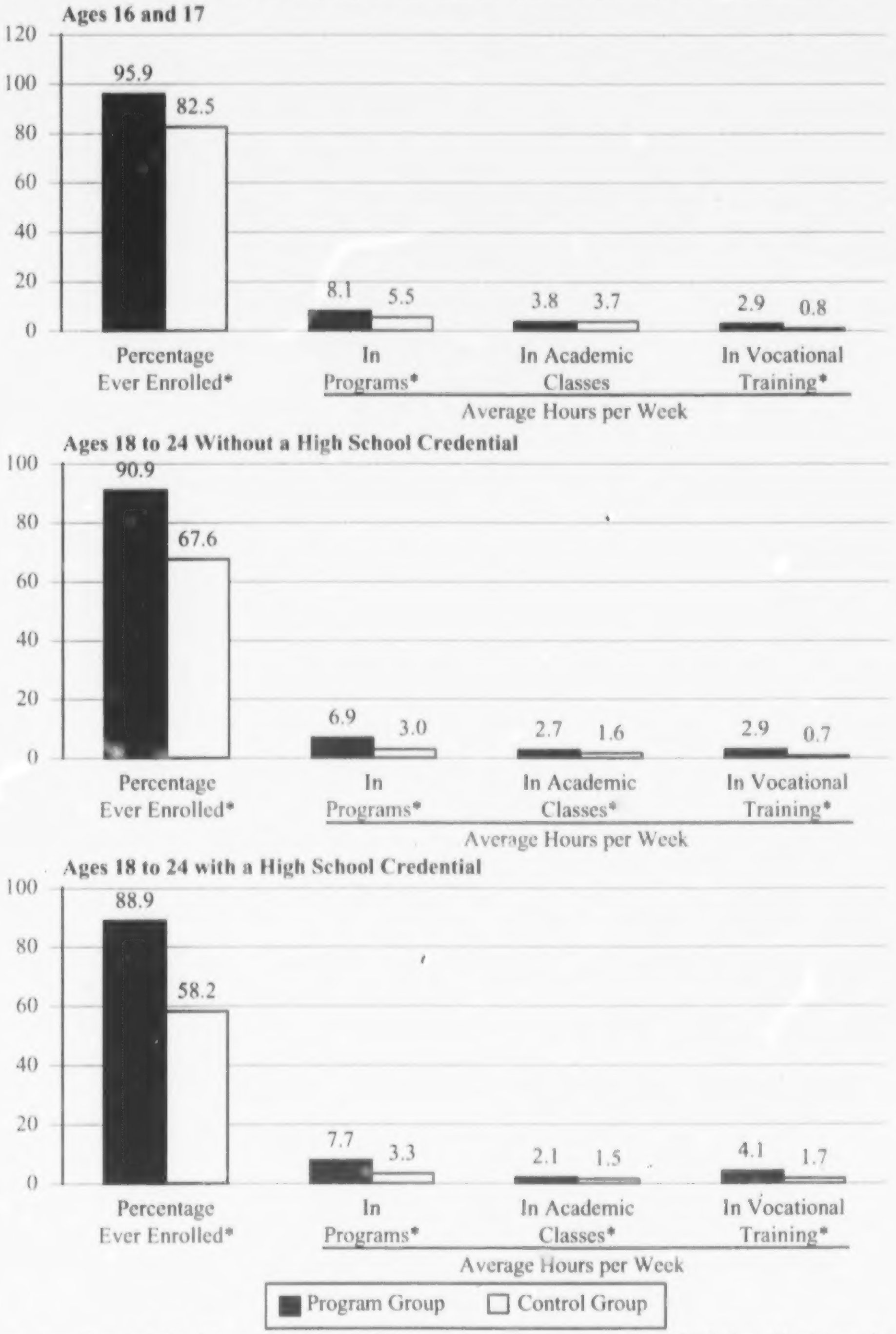
### b. Impact Findings

The impacts on overall measures of participation in education and training programs were very large for each subgroup (Figure V.8). However, they were somewhat smaller for the 16- and 17-year-olds because of high control group participation rates for this group. The impact per eligible applicant on hours per week spent in programs was about 2.6 hours per week (541 hours in total) for the youngest group and about 4 hours per week (832 hours in total) for the two older groups.

Impacts on time spent in academic classroom training were large and statistically significant for the older youth, but not for those 16 and 17 (Figure V.8). We find no impacts on time spent in academic classes for those 16 and 17, because many control group members in this group received intensive academic classroom instruction in high school and in GED programs. However, we find large positive impacts on the receipt of academic services for the two older groups, because the older control group members were less likely to participate in academic-intensive programs, whereas the older Job Corps participants in the program group received some academic instruction in Job Corps.

FIGURE V.8

PARTICIPATION AND HOURS PER WEEK IN EDUCATION AND TRAINING PROGRAMS, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE



Source: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

Impacts on time spent in vocational training, however, were very large and positive for each subgroup. Program group members typically received about three times more hours of vocational training than control group members.

Finally, for all age groups, we find large impacts on the receipt of certificates emphasized by Job Corps, but small differences by research status on the attainment of a high school diploma or college degree (Figure V.9). Impacts on the receipt of a GED were similarly large for both the younger and older dropouts. Although there were no impacts on time spent in academics for those 16 and 17, we find large impacts on the attainment of a GED for this group, because of the emphasis that Job Corps places on it. Impacts on the receipt of a high school diploma were negative, but small, for both dropout groups, because of the low rates of high school completion among the control group (only about 7.5 percent of all control group dropouts attained a diploma). Impacts on the receipt of a vocational certificate were also very large for all groups. Finally, at 48 months, Job Corps had no effect on the receipt of a two-year or four-year college degree for those who had a high school credential at baseline.

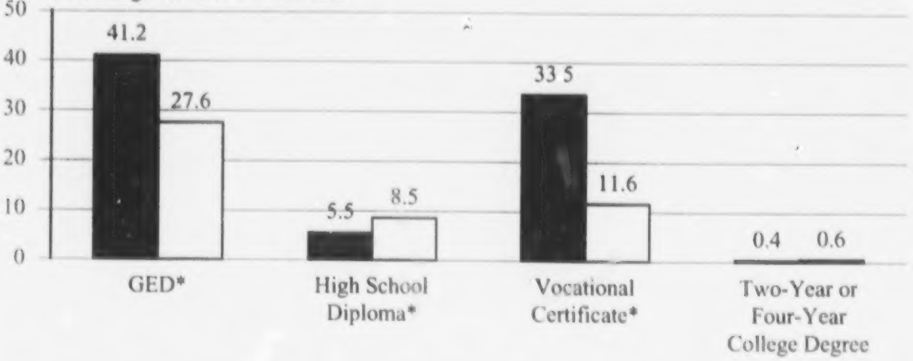
### 2. Impacts for Other Key Subgroups

Table C.7 presents impact results on selected education-related outcomes for each of the following subgroups: gender, residential designation status by gender, arrest history, race and ethnicity, and application date (whether before or after ZT policies took effect). Average control group measures and impacts on these outcome measures were remarkably similar across the subgroups. Thus, Job Corps leads to large increases in participation in education and training programs and in educational attainment across diverse groups of youths served by the program.

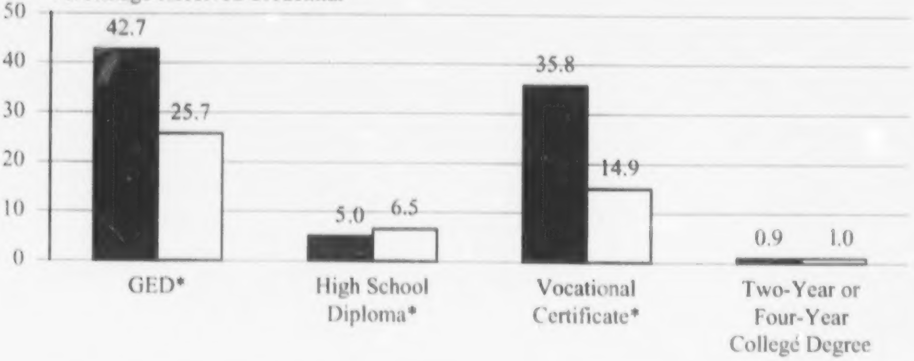
## FIGURE V.9

## EDUCATIONAL ATTAINMENT, BY AGE AND HIGH SCHOOL CREDENTIAL STATUS AT BASELINE

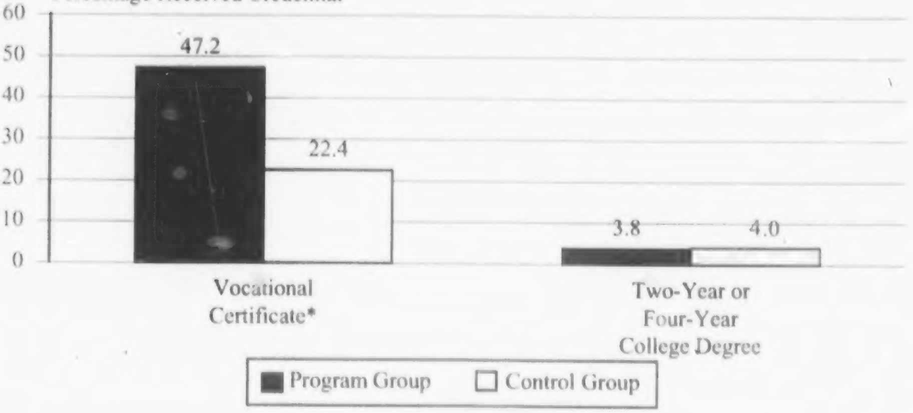
**Ages 16 and 17**
Percentage Received Credential



**Ages 18 to 24 Without a High School Credential**
Percentage Received Credential



**Ages 18 to 24 with a High School Credential**
Percentage Received Credential



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

Of particular note, we find similar impacts for those assigned to the residential and nonresidential component. This is consistent with our finding from the process analysis that nonresidential students are fully integrated into the academic and vocational components of Job Corps.

# VI. EMPLOYMENT AND EARNINGS

Chapter V showed that Job Corps participation leads to large impacts on time spent in academic classes and vocational training and on the attainment of GED and vocational certificates. In addition, Job Corps leads to increases in participants' functional literacy skills (Glazerman et al. 2000). Thus, Job Corps could increase participants' labor market productivity, which may in turn enhance their time spent employed, earnings, wage rates, and fringe benefits.

We expect negative impacts on participants' employment and earnings during the period of enrollment, because some participants would have held jobs if they had not gone to Job Corps. However, because of improvements in participants' skills, we expect positive impacts on employment and earnings after participants leave the program and after a period of readjustment. In light of the variation in the duration of program participation and the period of readjustment, it is difficult to predict when positive impacts are likely to emerge. Thus, we cannot predict in which month after random assignment the earnings of the program group were likely to have exceeded those of the control group.

This chapter presents program impacts on employment and earnings. It presents impacts for the full sample and for key subgroups during the 48 months after each youth was found eligible for Job Corps.

We find that Job Corps generated positive employment and earnings impacts beginning in the third year after random assignment, and that the impacts persisted through the end of the 48-month follow-up period. The employment and earnings of the control group were larger than those of the program group early in the follow-up period, because many program group members were enrolled in Job Corps then. It took about two years from random assignment for the earnings of the program group to overtake those of the control group. The impacts grew between quarters 8 and 12, and then remained fairly constant from quarters 13 to 16 (that is, they *persisted* in year 4). In year 4, average weekly earnings for program group members were $16 higher than for control group members ($211, compared to $195). The estimated impact per Job Corps *participant* was $22 per week (or $1,150 in total during year 4), which translates into a 12 percent gain in average weekly earnings due to program participation. These year 4 impacts are statistically significant at the 1 percent level.

Over the whole period, Job Corps participants earned about $3 per week (or $624 overall) more than they would have if they had not enrolled in Job Corps. This impact, however, is not statistically significant.

Job Corps also had positive effects on the employment rate and time spent employed beginning in year 3. As expected, the impacts on the employment measures were negative during the in-program period. They became positive in quarter 8, increased sharply between quarters 8 and 12, and remained fairly constant afterwards. In year 4, the average quarterly impact on the employment rate was about 3 percentage points per eligible applicant (69 percent for the program group, compared to 66 percent for the control group). The year 4 impact on hours employed per week was 1.4 hours per eligible applicant (27.4 hours for the program group, compared to 26 hours for the control group). This translates to an impact of nearly 2 hours per participant, or an 8 percent gain due to program participation. The year 4 impact per eligible applicant on the percentage of weeks employed was about 3 percentage points (60 percent, compared to 57 percent). These impact estimates are all statistically significant at the 1 percent level.

The earnings gains late in the period were due to a combination of greater hours of work and higher earnings per hour. We estimate that program group members earned about $11 more per week in year 4 than control group members because they worked more hours, and that they earned about $5 more per week because they had higher earnings per hour. These gains sum to the $16 impact on earnings per week in year 4.

Program group members secured higher-paying jobs with slightly more benefits in their most recent jobs in quarters 10 and 16. These findings are consistent with our findings from the literacy study (Glazerman et al. 2000) that Job Corps increases participants' skill levels and, hence, productivity. Employed program group members earned an average of $0.24 more per hour than employed control group members in their most recent job in quarter 10 ($6.77, compared to $6.53), and an average of $0.22 more per hour in their most recent job in quarter 16 ($7.55, compared to $7.33). Furthermore, the wage gains were similar across broad occupational categories, although similar percentages of program and control group members worked in each occupational area in both quarters. In addition, employed program group members were slightly more likely to hold jobs that offered fringe benefits (such as health insurance, retirement or pension benefits, paid sick leave, and paid vacation).

Positive impacts in the postprogram period were found broadly across most key subgroups of students. Beneficial program impacts were found for males and females, younger and older students, those with and without a high school credential at random assignment, and whites and African Americans (but not Hispanics).

Both the residential and the nonresidential program components were effective for the students they served. Earnings and employment impacts in years 3 and 4 were positive overall for those assigned to each component. Furthermore, employment and earnings gains were found for males, females with children, and females without children in each component, except for nonresidential females without children. Thus, the residential and nonresidential program components were effective for broad groups of students.

In the rest of this chapter, we present details of our findings on impacts on labor market outcomes. The next section discusses the impacts on employment rates, time employed, and earnings for all students. To provide insight on the nature and quality of the jobs held, we next compare the characteristics of jobs held by program and control group members. The third section presents impacts on the likelihood of being employed or engaging in educational activities (that is, engaging in an activity that improves a youth's long-run employment prospects). Finally, in the fourth section, we present impact findings for key subgroups. Appendix D contains supplementary tables.

## A. IMPACTS ON EMPLOYMENT RATES, TIME EMPLOYED, AND EARNINGS

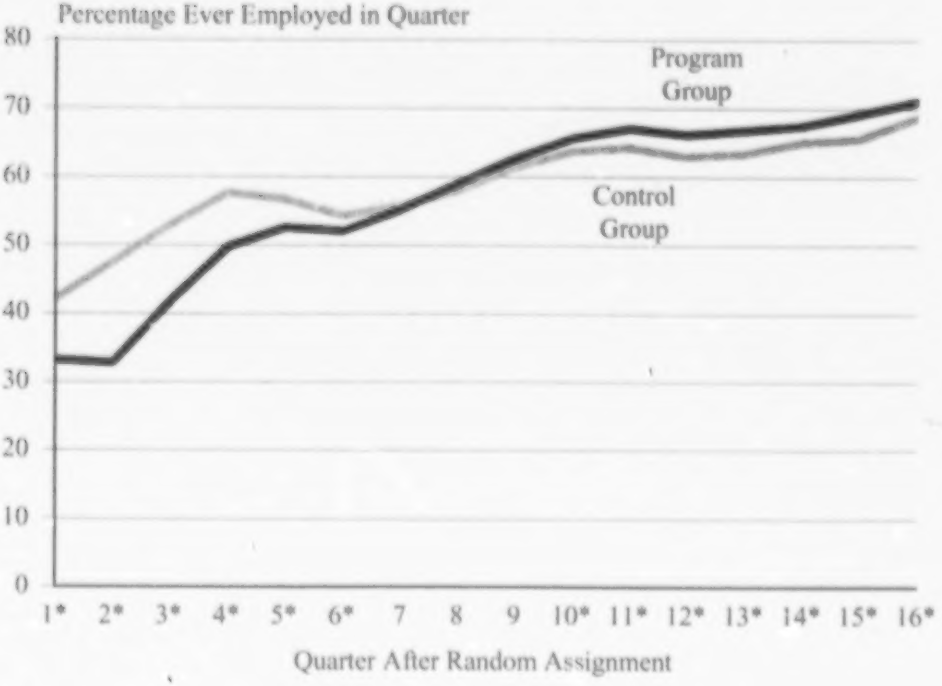
This section compares employment experiences of all control and program group members during the first 48 months after each applicant was determined eligible for Job Corps. We focus on the last two years of the observation period, because most enrollees in the program group had left Job Corps by then.

### 1. Impacts on Employment Rates

Figure VI.1 displays the proportion of all program and control group members who were ever employed during each quarter (3-month period) over the 48-month period after random assignment. The quarterly employment rates of the control group show what program group members would have experienced if they had not had the opportunity to enroll in Job Corps. The differences between the quarterly employment rates of the program and the control group are estimated impacts per eligible applicant. Asterisks along the x-axis indicate the statistical significance of the impact estimates. Table VI.1 displays the calculations and also shows impacts per participant.

FIGURE VI.1

EMPLOYMENT RATES, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statisticall significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VI.1

## IMPACTS ON EMPLOYMENT RATES AND THE NUMBER OF JOBS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter After Random Assignment | | | | | | |
| 1 | 33.2 | 42.1 | -8.9*** | 28.1 | -12.4*** | -30.6 |
| 2 | 32.8 | 47.5 | -14.7*** | 25.8 | -20.4*** | -44.2 |
| 3 | 41.8 | 53.0 | -11.1*** | 36.6 | -15.4*** | -29.6 |
| 4 | 49.8 | 57.7 | -7.9*** | 46.3 | -10.9*** | -19.1 |
| 5 | 52.6 | 56.7 | -4.1*** | 50.8 | -5.7*** | -10.1 |
| 6 | 52.1 | 54.3 | -2.2** | 51.1 | -3.0** | -5.6 |
| 7 | 55.2 | 55.8 | -0.6 | 54.5 | -0.8 | -1.5 |
| 8 | 59.0 | 57.9 | 1.2 | 59.0 | 1.6 | 2.8 |
| 9 | 62.7 | 61.4 | 1.2 | 63.3 | 1.7 | 2.7 |
| 10 | 65.6 | 63.7 | 1.9** | 66.5 | 2.7** | 4.2 |
| 11 | 67.1 | 64.3 | 2.9*** | 67.7 | 4.0*** | 6.2 |
| 12 | 66.2 | 63.0 | 3.2*** | 66.3 | 4.4*** | 7.1 |
| 13 | 66.8 | 63.4 | 3.4*** | 67.3 | 4.8*** | 7.6 |
| 14 | 67.5 | 65.1 | 2.4*** | 67.9 | 3.3*** | 5.1 |
| 15 | 69.2 | 65.6 | 3.6*** | 70.1 | 5.0*** | 7.7 |
| 16 | 71.1 | 68.7 | 2.4*** | 71.6 | 3.3*** | 4.9 |
| Percentage Employed at 48 Months | 62.1 | 59.1 | 3.0*** | 62.5 | 4.2*** | 7.1 |
| Percentage Ever Employed | 95.8 | 95.0 | 0.7* | 96.0 | 1.0* | 1.1 |
| Number of Jobs (Percentages) | | | | | | |
| 0 | 4.7 | 5.3 | -0.7 | 4.4 | -1.0 | -17.8 |
| 1 | 11.6 | 11.7 | -0.1 | 11.6 | -0.1 | -1.2 |
| 2 | 18.1 | 17.3 | 0.8 | 18.4 | 1.1 | 6.4 |
| 3 | 18.4 | 18.8 | -0.4 | 18.6 | -0.5 | -2.7 |
| 4 or more | 47.3 | 46.9 | 0.4 | 47.0 | 0.5 | 1.1 |
| Average Number of Jobs | 3.6 | 3.6 | 0.0 | 3.6 | 0.0 | -1.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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The employment rate of the control group increased over time. It was 42 percent in quarter 1, 58 percent in quarter 8, 63 percent in quarter 12, and 69 percent in quarter 16. Employment increased as the youths left school and gained work experience.[1]

The employment rate of the control group was significantly higher than that of the program group (impacts were negative) during the period when many program group members were enrolled in Job Corps. The differences narrowed over time as some program group enrollees started to leave Job Corps and take jobs. Impacts became positive by quarter 8 (that is, two years after random assignment). For example, the employment rate was about 9 percentage points lower for the program group than for the control group in quarter 1 (33 percent, compared to 42 percent), about 4 percentage points lower in quarter 5, and about 1 percentage point higher in quarter 8.

The impact per eligible applicant on the employment rate nearly tripled, from 1.2 percentage points in quarter 8 to 3.2 percentage points in quarter 12, and remained fairly constant at about 3 percentage points between quarters 12 and 16. The impact *per participant* was about 4 percentage points during the fourth year after random assignment (that is, during year 4). The quarterly impacts were statistically significant at the 5 percent level starting in quarter 10.

Nearly all sample members in both the program and the control groups (about 95 percent) worked at some point during the 48-month period (Table VI.1). The distribution of the number of jobs held by the two groups is very similar. Nearly half of each group had four or more jobs during the 48-month period, and only 12 percent had only one job. Thus, job turnover was common for both groups.

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[1] The employment rate was 43 percent in the quarter prior to random assignment and 43.5 percent in the quarter before that.

### 2. Impacts on Time Employed

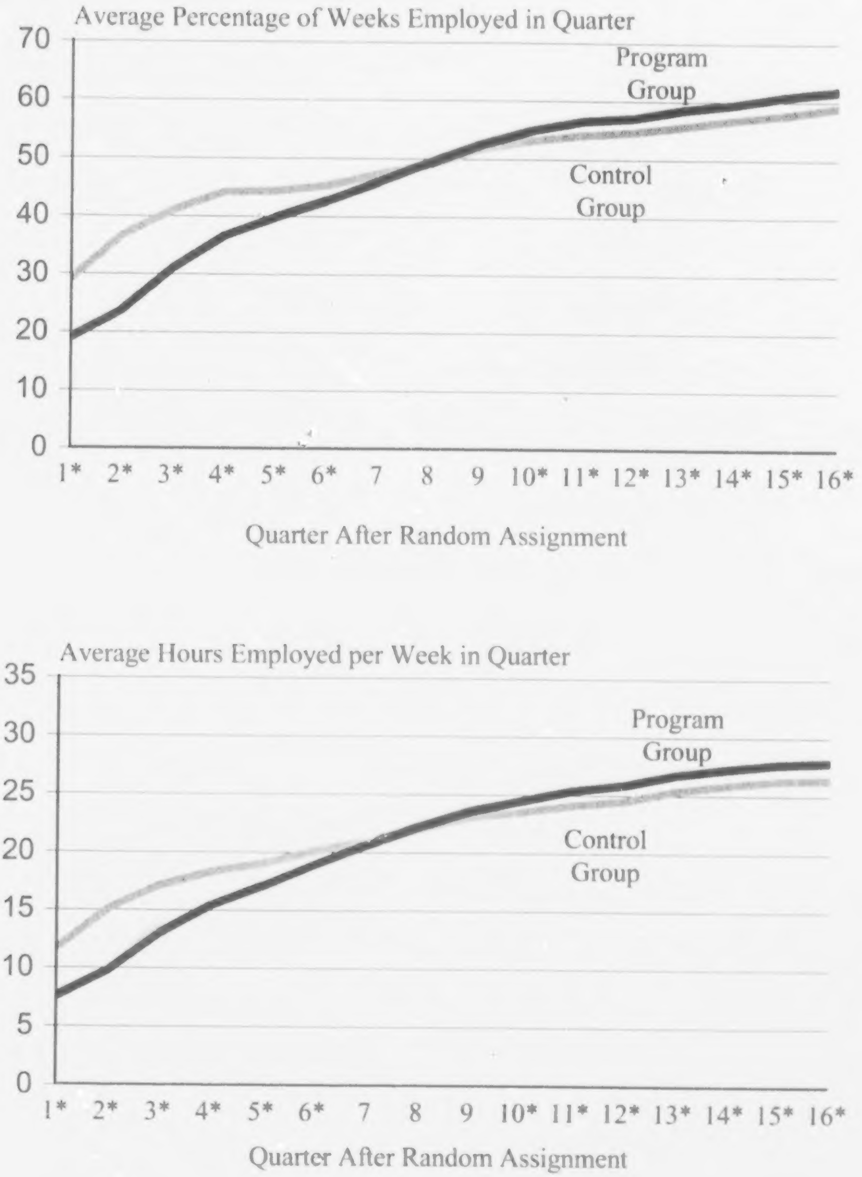
We used two measures of the time that sample members were employed during a given period: (1) the proportion of weeks employed, and (2) the number of hours worked per week. We calculated the proportion of weeks employed by dividing the total number of weeks that each youth was employed during the period by the number of weeks in the period (for example, 13 weeks for a quarter and 208 weeks for the entire 48-month period). Similarly, we calculated hours worked per week by dividing the total number of hours that the youth worked during the period by the number of weeks in the period. The measures were set to 0 for those who were not employed during the period.

Not surprisingly, the profile of the quarterly-time-employed measures follows a pattern similar to that of the quarterly employment rates (Figure VI.2 and Tables VI.2 and VI.3). Impacts were negative and statistically significant during quarters 1 to 6 and became positive in quarter 8 (about two years after random assignment). For example, the average hours worked per week during quarter 1 was about 12 hours for control group members and 8 hours for program group members (an impact of -4 hours per week). The impact on hours worked per week was -1.9 hours in quarter 5 and 0.2 hours in quarter 8.

The positive impacts on weeks and hours employed increased sharply between quarters 8 and 12, and then remained fairly constant through quarter 16. The impacts were statistically significant at the 5 percent level starting in quarter 10 (that is, after two and a half years after random assignment). Program group members were employed for an average of about 60 percent of weeks

## FIGURE VI.2

## TIME EMPLOYED, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VI.2

## IMPACTS ON THE PERCENTAGE OF WEEKS EMPLOYED

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Percentage of Weeks Employed, by Quarter After Random Assignment | | | | | | |
| 1 | 19.1 | 29.2 | -10.0*** | 14.0 | -13.9*** | -49.8 |
| 2 | 23.7 | 36.6 | -12.9*** | 17.5 | -17.9*** | -50.6 |
| 3 | 31.1 | 41.0 | -9.9*** | 25.9 | -13.8*** | -34.7 |
| 4 | 36.6 | 44.1 | -7.4*** | 33.0 | -10.4*** | -23.9 |
| 5 | 39.9 | 44.4 | -4.4*** | 37.6 | -6.2*** | -14.1 |
| 6 | 42.7 | 45.3 | -2.6*** | 41.2 | -3.6*** | -8.1 |
| 7 | 46.0 | 47.3 | -1.3 | 45.2 | -1.8 | -3.8 |
| 8 | 49.3 | 49.1 | 0.2 | 48.7 | 0.2 | 0.5 |
| 9 | 52.5 | 51.8 | 0.7 | 52.6 | 1.0 | 1.9 |
| 10 | 55.0 | 53.2 | 1.8** | 55.6 | 2.5** | 4.7 |
| 11 | 56.6 | 54.1 | 2.4*** | 56.9 | 3.4*** | 6.3 |
| 12 | 57.1 | 54.7 | 2.5*** | 57.4 | 3.4*** | 6.4 |
| 13 | 58.6 | 55.7 | 3.0*** | 59.0 | 4.1*** | 7.5 |
| 14 | 59.6 | 56.8 | 2.9*** | 60.1 | 4.0*** | 7.1 |
| 15 | 60.9 | 57.7 | 3.2*** | 61.4 | 4.4*** | 7.8 |
| 16 | 61.8 | 59.0 | 2.8*** | 62.3 | 3.9*** | 6.6 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1 | 27.6 | 37.8 | -10.2*** | 22.8 | -14.2*** | -38.4 |
| 2 | 44.7 | 46.8 | -2.1*** | 43.4 | -2.9*** | -6.3 |
| 3 | 55.2 | 53.5 | 1.7** | 55.5 | 2.4** | 4.5 |
| 4 | 60.2 | 57.2 | 3.0*** | 60.6 | 4.1*** | 7.3 |
| Percentage of Weeks Employed During the Entire 48-Month Period | | | | | | |
| 0 | 5.0 | 5.8 | -0.8***[d] | 4.7 | -1.2***[d] | -20.0 |
| 0 to 10 | 8.1 | 8.4 | -0.3 | 8.1 | -0.4 | -5.2 |
| 10 to 25 | 13.9 | 13.5 | 0.4 | 14.6 | 0.5 | 3.9 |
| 25 to 50 | 27.1 | 25.0 | 2.1 | 28.2 | 3.0 | 11.8 |
| 50 to 75 | 27.0 | 23.3 | 3.7 | 28.1 | 5.2 | 22.4 |
| 75 or more | 19.0 | 24.1 | -5.1 | 16.3 | -7.1 | -30.3 |
| Average Percentage of Weeks Employed During the Entire 48-Month Period | 45.2 | 46.9 | -1.7*** | 44.0 | -2.4*** | -5.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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## TABLE VI.3

## IMPACTS ON HOURS EMPLOYED PER WEEK

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Hours Employed per Week, by Quarter After Random Assignment | | | | | | |
| 1 | 7.6 | 11.7 | -4.1*** | 5.4 | -5.7*** | -51.2 |
| 2 | 9.8 | 15.1 | -5.4*** | 7.1 | -7.4*** | -51.2 |
| 3 | 13.0 | 17.1 | -4.1*** | 10.7 | -5.7*** | -34.9 |
| 4 | 15.4 | 18.3 | -2.8*** | 13.8 | -3.9*** | -22.0 |
| 5 | 17.1 | 19.0 | -1.9*** | 16.1 | -2.7*** | -14.2 |
| 6 | 18.9 | 20.1 | -1.2*** | 18.2 | -1.7*** | -8.5 |
| 7 | 20.6 | 21.0 | -0.4 | 20.3 | -0.5 | -2.5 |
| 8 | 22.2 | 22.0 | 0.2 | 22.1 | 0.3 | 1.3 |
| 9 | 23.6 | 23.1 | 0.5 | 23.8 | 0.7 | 3.0 |
| 10 | 24.5 | 23.5 | 1.0** | 25.0 | 1.3** | 5.6 |
| 11 | 25.4 | 24.1 | 1.3*** | 25.8 | 1.9*** | 7.7 |
| 12 | 25.9 | 24.5 | 1.4*** | 26.3 | 1.9*** | 8.0 |
| 13 | 26.8 | 25.4 | 1.5*** | 27.2 | 2.0*** | 8.1 |
| 14 | 27.3 | 25.9 | 1.4*** | 27.6 | 1.9*** | 7.3 |
| 15 | 27.7 | 26.3 | 1.5*** | 28.0 | 2.0*** | 7.8 |
| 16 | 27.9 | 26.4 | 1.5*** | 28.1 | 2.0*** | 7.8 |
| Average Hours Employed per Week, by Year | | | | | | |
| 1 | 11.4 | 15.5 | -4.1*** | 9.3 | -5.8*** | -38.2 |
| 2 | 19.7 | 20.5 | -0.9** | 19.1 | -1.2** | -5.9 |
| 3 | 24.7 | 23.7 | 1.0*** | 25.1 | 1.4*** | 6.1 |
| 4 | 27.4 | 26.0 | 1.4*** | 27.7 | 1.9*** | 7.6 |
| Hours Employed per Week During the Entire 48-Month Period (Percentage) | | | | | | |
| 0 | 5.1 | 5.9 | -0.8***[d] | 4.8 | -1.2***[d] | -19.7 |
| 0 to 5 | 11.2 | 11.6 | -0.4 | 11.3 | -0.5 | -4.6 |
| 5 to 15 | 24.4 | 23.8 | 0.6 | 25.1 | 0.8 | 3.2 |
| 15 to 25 | 23.1 | 20.9 | 2.2 | 24.2 | 3.1 | 14.8 |
| 25 to 35 | 19.8 | 19.0 | 0.9 | 20.1 | 1.2 | 6.3 |
| 35 or more | 16.4 | 18.8 | -2.4 | 14.5 | -3.4 | -18.8 |
| Average Hours Employed per Week During the Entire 48-Month Period | 20.5 | 21.1 | -0.5** | 20.1 | -0.8** | -3.6 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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in year 4, compared to 57 percent of weeks for control group members. Similarly, the average weekly hours worked per eligible applicant increased from 26 to 27.4 hours during this period. These differences translate to increases of about 7.5 percent in the weeks and hours worked by Job Corps participants.

Over the entire 48-month period, control group members worked slightly more than program group members, who spent more time in education and training programs and whose employment rate did not "overtake" that of the control group until quarter 8. Control group members spent an average of about 47 percent of weeks employed, compared to about 45 percent for program group members (a statistically significant impact of about -2 percentage points, or about 4 weeks over 48 months). Similarly, the average control group member worked 0.5 hours per week more than the average program group member, or about 100 hours more over the entire 48-month period.

### 3. Impacts on Earnings

Earnings are the most comprehensive employment-related measure, because they reflect both work effort and earnings per hour. To examine earnings impacts, we calculated period-specific earnings per week from all jobs for each sample member. We calculated earnings per week by dividing total period earnings by the number of weeks in the period. Thus, the measure represents the earnings (in 1995 dollars) of a youth in a typical week during the period.[2]

Earnings per week increased over time for the control group (Figure VI.3 and Table VI.4). For example, control group members earned an average of $66 per week in quarter 1, $147 per week in quarter 8, $179 per week in quarter 12, and $199 per week in quarter 16. Earnings increased because

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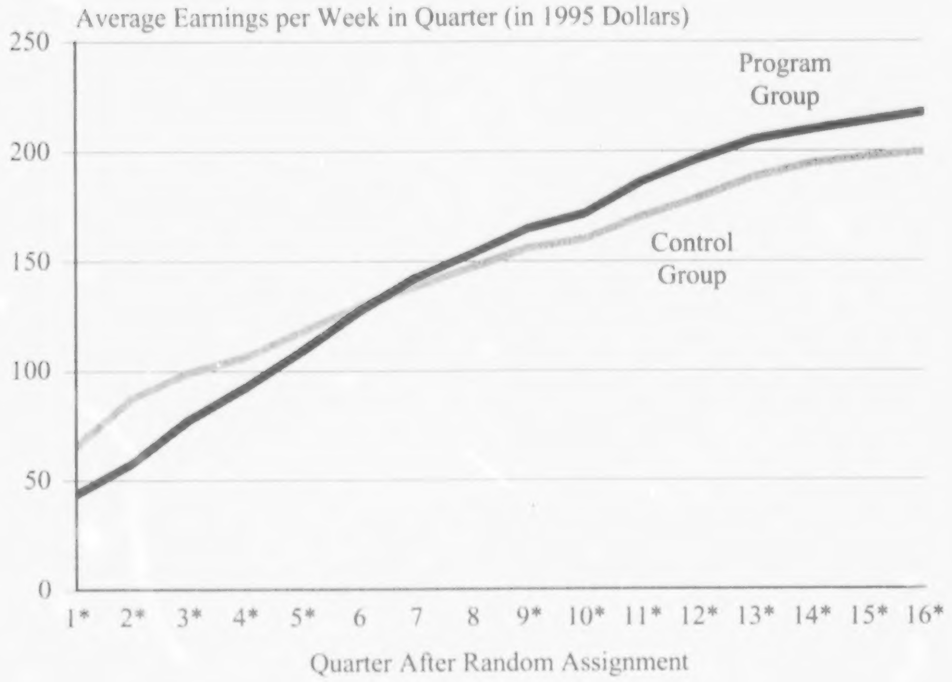
[2] We measure earnings in 1995 dollars to be consistent with our measure of program costs used in the benefit-cost analysis (McConnell et al. 2001). We use primarily program costs in PY 1995 because that was the period when most program group participants entered Job Corps.

FIGURE VI.3

AVERAGE EARNINGS PER WEEK, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VI.4

## IMPACTS ON EARNINGS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Quarter After Random Assignment (in 1995 Dollars) | | | | | | |
| 1 | 43.5 | 65.5 | -22.0*** | 30.8 | -30.6*** | -49.9 |
| 2 | 57.9 | 87.4 | -29.5*** | 41.4 | -41.0*** | -49.7 |
| 3 | 77.6 | 99.2 | -21.6*** | 63.3 | -30.1*** | -32.2 |
| 4 | 92.4 | 106.0 | -13.6*** | 81.6 | -19.0*** | -18.9 |
| 5 | 108.8 | 117.7 | -8.9*** | 102.0 | -12.3*** | -10.8 |
| 6 | 126.8 | 129.3 | -2.5 | 122.5 | -3.4 | -2.7 |
| 7 | 142.3 | 138.2 | 4.1 | 139.6 | 5.8 | 4.3 |
| 8 | 153.3 | 146.9 | 6.4* | 151.7 | 8.9* | 6.2 |
| 9 | 164.8 | 155.8 | 9.0** | 165.0 | 12.5** | 8.2 |
| 10 | 171.6 | 160.0 | 11.6*** | 174.6 | 16.2*** | 10.2 |
| 11 | 186.1 | 170.2 | 15.9*** | 188.2 | 22.1*** | 13.3 |
| 12 | 196.2 | 178.6 | 17.6*** | 198.4 | 24.5*** | 14.1 |
| 13 | 205.3 | 188.0 | 17.3*** | 208.4 | 24.1*** | 13.1 |
| 14 | 209.8 | 194.2 | 15.7*** | 212.4 | 21.8*** | 11.4 |
| 15 | 213.7 | 197.2 | 16.5*** | 216.0 | 22.9*** | 11.9 |
| 16 | 217.5 | 199.4 | 18.1*** | 218.4 | 25.2*** | 13.0 |
| Average Earnings Per Week, by Year | | | | | | |
| 1 | 67.6 | 89.6 | -22.1*** | 54.8 | -30.7*** | -35.9 |
| 2 | 132.2 | 133.3 | -1.1 | 128.0 | -1.5 | -1.2 |
| 3 | 178.6 | 165.2 | 13.4*** | 180.3 | 18.6*** | 11.5 |
| 4 | 211.4 | 195.4 | 15.9*** | 213.0 | 22.1*** | 11.6 |
| Earnings per Week During the Entire 48-Month Period (Percentage) | | | | | | |
| 0 | 3.8 | 4.4 | -0.6 | 3.6 | -0.8 | -18.8 |
| 1 to 25 | 11.3 | 12.7 | -1.4 | 11.0 | -2.0 | -15.2 |
| 25 to 75 | 19.3 | 19.5 | -0.2 | 19.9 | -0.2 | -1.1 |
| 75 to 150 | 24.6 | 23.7 | 1.0 | 25.5 | 1.4 | 5.7 |
| 150 to 225 | 19.0 | 18.7 | 0.4 | 19.3 | 0.5 | 2.8 |
| 225 or more | 21.9 | 21.1 | 0.8 | 20.6 | 1.1 | 5.7 |
| Average Total Earnings per Week During the Entire 48-Month Period | 143.4 | 141.3 | 2.0 | 140.4 | 2.8 | 2.1 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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both hours worked and hourly wage rates increased as the youths left school and gained work experience.

Interestingly, control group earnings decreased in the recent period prior to random assignment (not shown). Average earnings per week were $49 in the quarter prior to random assignment and $62 in the quarter before that. This preprogram dip in earnings could have come about because youths worked less in anticipation of enrolling in Job Corps, or because they had particularly poor labor market experiences (which could have induced them to apply to Job Corps).[3]

The general pattern of the earnings impacts over time is similar to that of the employment impacts. However, positive impacts on earnings emerged earlier, and the earnings impacts were larger in years 3 and 4. Average weekly earnings were significantly higher for control group members than for program group members during the first five quarters after random assignment. The impacts were most negative in quarters 1 to 3 and became smaller in quarters 4 to 6, as participants started leaving Job Corps. Control group members earned an average of about $22 more per week during quarter 1, $14 more per week during quarter 4, and less than $9 more per week during quarter 5.

Earnings impacts became positive in quarter 7 and continued to grow in quarters 8 to 12. They remained fairly constant from quarters 12 to 16 (that is, they *persisted* in year 4). The impacts were statistically significant at the 5 percent level after quarter 8. In year 4, program group members earned an average of about $211 per week, compared to $195 per week for control group members. This $16 impact per eligible applicant translates to a $22 impact per program participant. In year

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[3] The earnings dip occurred for all age groups, although the dip was larger for the older youths. Average earnings per week decreased from $33 to $28 for those 16 and 17, and from $97 to $72 for those 20 to 24.

4, participants earned an average of about $1,150 (or 12 percent) more than they would have if they had not enrolled in the program.

The estimated impact per participant on earnings over the whole 48-month period was about $3 per week ($624 overall). This impact is not statistically significant.

It is noteworthy that, as discussed in Chapter V, similar percentages of program and control group members were in education and training programs in years 3 and 4, and only 13 percent of both groups were in programs in the last week in month 48. Consequently, it is unlikely that the postprogram earnings and employment impact estimates were greatly affected by differences across the research groups in school enrollment rates.

### 4. Decomposition of Impacts on Earnings in Year 4 into Its Components

Earnings over a given period are the product of hours worked during the period and earnings per hour. As discussed, we find positive impacts on both earnings and hours worked in year 4. We also find a positive impact of $0.20 on earnings per hour in year 4 ($7.72 for the program group and $7.52 for the control group).[4]

To assess the extent to which the earnings impact was due to the impact on hours worked and how much was due to the impact on hourly earnings, we express average earnings per week for program group members as follows:

$$(1) \quad \bar{E}_P = \frac{\bar{E}_P}{\bar{H}_P}\bar{H}_P = \bar{W}_P\bar{H}_P,$$

---

[4] We calculated the $0.20 impact using Tables VI.3 and VI.4 and noting that average hourly earnings in year 4 were $7.72 ($211.4 earned/27.4 hours worked) for the program group and $7.52 ($195.4 earned/26.0 hours worked) for the control group.

where $\bar{E}_P$ is average earnings per week for the program group, $\bar{H}_P$ is average hours worked per week, and $\bar{W}_P$ is hourly earnings (that is, average earnings divided by average hours).[5] Average earnings for the control group can be written in the same way, and thus impacts on earnings per week can be expressed as follows:

$$(2) \quad (\bar{E}_P - \bar{E}_C) = \bar{W}_P\bar{H}_P - \bar{W}_C\bar{H}_C.$$

If we add and subtract the term $\bar{W}_P\bar{H}_C$ in equation (2) and rearrange terms, then equation (2) becomes:

$$(3) \quad (\bar{E}_P - \bar{E}_C) = \bar{W}_P(\bar{H}_P - \bar{H}_C) + \bar{H}_C(\bar{W}_P - \bar{W}_C).$$

Equation (3) decomposes the impact on earnings into a weighted average of the impact on hours employed per week and the impact on hourly earnings, where the weights are average hourly earnings for the program group and average hours worked per week for the control group, respectively.[6]

Using equation (3), we find that about two-thirds of the earnings impact in year 4 was due to the impact on hours worked and that one-third was due to the impact on earnings per hour. Stated another way, program group members earned about $11 more per week because they worked more hours, and earned about $5 more per week because they had higher earnings per hour.

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[5] This expression is only an *approximation* to the average wage received by the program group, because to calculate the average wage, it would be necessary to divide earnings by hours worked for *each* youth, and then take the average of these individual values. This procedure is difficult to implement for those who did not work (because we would be dividing by zero hours worked). In Section B below, we discuss hourly wages for those employed in quarter 16.

[6] One can instead add and subtract the term $\bar{W}_C\bar{H}_P$ from equation (2) to derive a slightly different set of weights in equation (3). We obtained the same conclusions using either approach.

### 5. The Overtaking Point

Average program group earnings overtook average control group earnings in quarter 7, and the overtaking point for the employment rate and hours worked was in quarter 8. Thus, it took nearly two years until positive employment-related impacts emerged.

The average program group participant enrolled in Job Corps about 1.4 months after random assignment and remained in the program for eight months. Thus, by quarter 4, the typical program member had left Job Corps. Why did a full year elapse between the time an average participant left Job Corps and the overtaking point?

Many factors could have influenced the timing of the "overtaking point" (the point at which program impacts became positive) for the employment and earnings outcomes. The timing of the overtaking point was due in part to (1) the length of time that each participant spent in the program, (2) the length of time until the potential gains from participation were realized in the form of more work and better jobs, (3) the size of the gain for each student, and (4) the interaction among these three factors. However, these same factors also affected the outcomes of the control group, because, as discussed, many of these youth also enrolled in education programs. Furthermore, sample members participated in programs at different points during the follow-up period because they entered their programs at different points and had different durations of stay. Thus, it is very difficult to disentangle the factors that can explain the timing of the overtaking point.

However, we offer several possible reasons that positive program impacts on the employment and earnings outcomes did not occur until about two years after random assignment. First, impacts on participation in education programs were relatively large until quarter 7, primarily because of intensive program group participation in Job Corps. For example, in quarter 6, the impact per participant on the enrollment rate in education programs was about 8 percentage points, and about 14 percent of program group participants were still in Job Corps. Second, it took time for some participants to find jobs after they left the program. For example, in the year after leaving the program, about 21 percent of participants did not work, and 16 percent first worked more than six months after leaving.[7] In addition, about 30 percent of program terminees enrolled in another education program during the one-year period. To be sure, control group members may have also had a period of readjustment after they left their programs. However, for Job Corps participants, this period may have been longer, because most were residential students and had been away from home for a relatively long time.

### 6. Effects of the Strong Economy

The 48-month follow-up data cover the period from November 1994 to February 2000, a period of strong economic growth. The unemployment rate for the civilian population of those 16 and older was 5.5 percent in late 1994, which was low by recent historical standards. The rate decreased to about 4.5 percent in mid-1998 and to about 4 percent in early 2000. Similarly, the unemployment rate for those 16 to 19 decreased from about 17 percent to under 14 percent during the same period. In addition, inflation was low.

It is impossible to know whether employment and earnings impacts would have differed in a weaker economy. Employment rates and earnings were probably higher in the strong economy than they would have been in a weaker one. However, they were likely to have been higher for *both* program and control group members.

There is some evidence that the strong economy increased average earnings more for the control group than the program group. This is because the control group typically had less training and lower skills, and the literature suggests that those with lower skills benefit more from a tight labor market than those with higher skills (Hoynes 1999; and Katz and Krueger 1999). Thus, although both program and control group members earned low wages, the strong economy may have favored the control group because more of them had lower skills. This would suggest that our employment and earnings impacts may be smaller than those that would have been obtained in a weaker economy.

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[7] These figures were calculated using only program group members who enrolled in Job Corps and who left the program at least a year before month 48 (that is, those who left before month 36).

We believe, however, that our impact estimates are probably representative of program effects generally. Unemployment rates are high for disadvantaged youth even in good economic times. In addition, the differences in skill levels between the program and control groups are small relative to the differences between high-skilled and low-skilled workers economywide. Consequently, it seems likely any advantage for the control group was small.

## B. DIFFERENCES IN HOURLY WAGES AND OTHER JOB CHARACTERISTICS

In this section, we examine the hourly wage and other characteristics of jobs held by program and control group members during quarters 10 and 16, including job tenure, usual hours worked per week, weekly earnings, occupations, types of employers, and available fringe benefits. We examine job characteristics at two time points to assess changes over time.

The analysis uses information on the most recent jobs held by sample members during the 10th and 16th quarters after random assignment. Youth who were not employed in quarter 10 were excluded from the quarter 10 analysis, and similarly for the quarter 16 analysis. Because we included only employed sample members in this analysis, and because Job Corps participation affected employment rates, and hence, which people were employed, differences in job characteristics should not be interpreted as impacts of the program.

To clarify this limitation, suppose that employment gains due to participation in Job Corps were concentrated among students who had lesser skills and ability and received lower wages. In this case, the employed program group would include a higher proportion of lower-skill/lower-wage workers than the employed control group. Consequently, differences in the average hourly wage rates of employed program and employed control group members would be a downwardly biased estimate of the true impact of Job Corps on the hourly wage rate of a particular participant.

To investigate whether the offer of Job Corps participation might have resulted in differences in the characteristics of employed sample members, we compared baseline characteristics and pre-program experiences of program and control group members who worked in quarters 10 and 16. The observable characteristics of workers in the program and control groups were similar on average (not shown). To be sure, some *unmeasured* differences between the two groups may have been correlated with the types of jobs held by the youths. In our judgment, however, simple program and control group comparisons are suggestive of program impacts on the characteristics of jobs held by participants, although these estimates should be interpreted with caution. To reinforce this distinction, we do not refer to these differences as impacts. In addition, we present differences per eligible applicant but not per program participant, because the assumptions needed to obtain estimates for participants are less tenable for these outcomes, which are conditional on other outcomes.

The comparisons lead to several conclusions:

- The average hourly wage rate in both quarters was about $0.23 higher for the employed program group than for the employed control group.
- Job Corps did not alter the distribution of workers across broad occupational categories, and the wage gains were similar across these broad occupations.
- Employed program group members in both quarters were more likely to hold jobs that offered fringe benefits.

Thus, the evidence suggests that program group members secured higher-paying jobs with more benefits, and that the effects persisted during the postprogram period. These findings are consistent with our finding that average functional literacy and numeracy levels were higher for the program group than the control group at 30 months (Glazerman et al. 2000), which suggests that labor market productivity was typically higher for program group members.

### 1. Differences in Job Tenure, Hours Worked, Hourly Wages, and Weekly Earnings

A higher percentage of program group than control group members were employed in quarter 10 (66 percent, compared to 64 percent) and in quarter 16 (71 percent, compared to 69 percent) (Table VI.5).[8] Only these workers were used in the analysis.

Most employed youths in both quarters had held their jobs for a short time, although, as expected, job tenure was typically longer in quarter 16. In quarter 10, average job tenure was 8.7 months for the employed control group, compared to 7.9 months for the employed program group. This difference reflects the longer time program group members spent in training. In quarter 16, average job tenure was 12 months for employed youths in both groups, and about 45 percent had been on their jobs for less than 6 months. The finding that many youths had short job tenure is consistent with our finding that many of them held several jobs during the 48-month period, which suggests that job turnover was common.

Most employed youths in both research groups were employed full-time. On average, program and control group members worked more than 40 hours per week in both quarters, and about 85 percent worked at least 30 hours. The small difference in hours worked by research status suggests that program impacts on hours worked (including workers *and* nonworkers) were due to program impacts on the employment rate and not to differences in work effort for those employed.

Employed control group members earned an average of $6.53 per hour in quarter 10 and $7.33 per hour in quarter 16. Hourly wages were low for most employed control group members, although

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[8] About three-quarters of those employed in quarter 16 were also employed in quarter 10.

## TABLE VI.5

## EMPLOYMENT TENURE, HOURS, AND HOURLY WAGES IN THE MOST RECENT JOB IN QUARTERS 10 AND 16

(Percentages)

| | Quarter 10 | | | Quarter 16 | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Program Group | Control Group | Difference | Program Group | Control Group | Difference |
| Employed in Quarter | 65.6 | 63.7 | 1.9** | 71.1 | 68.7 | 2.4*** |
| Number of Months on Job[a] | | | | | | |
| Less than 1 | 11.1 | 11.3 | -0.2***[b] | 9.8 | 10.5 | -0.6 |
| 1 to 3 | 21.5 | 20.4 | 1.1 | 16.3 | 17.3 | -0.9 |
| 3 to 6 | 21.8 | 20.0 | 1.9 | 17.6 | 17.0 | 0.6 |
| 6 to 12 | 20.7 | 19.6 | 1.1 | 19.6 | 18.6 | 1.0 |
| 12 to 24 | 19.5 | 20.9 | -1.3 | 20.9 | 20.6 | 0.2 |
| 24 or more | 5.3 | 7.8 | -2.6 | 15.7 | 16.0 | -0.3 |
| (Average months) | 7.9 | 8.7 | -0.8 | 11.7 | 11.8 | -0.1 |
| Usual Hours Worked per Week[a] | | | | | | |
| Less than 20 | 4.2 | 5.6 | -1.4 | 4.4 | 4.9 | -0.5 |
| 20 to 30 | 9.8 | 10.0 | -0.2 | 7.0 | 7.5 | -0.5 |
| 30 to 39 | 13.6 | 15.3 | -1.7 | 12.5 | 12.0 | 0.6 |
| 40 | 35.8 | 33.5 | 2.3 | 36.5 | 35.9 | 0.7 |
| More than 40 | 36.6 | 35.6 | 1.0 | 39.5 | 39.8 | -0.2 |
| (Average hours) | 41.7 | 40.9 | 0.8** | 42.8 | 42.4 | 0.4 |
| Hourly Wage[a] | | | | | | |
| Less than $4.50 | 5.8 | 7.1 | -1.2***[b] | 5.5 | 5.7 | -0.2***[b] |
| $4.50 to $6.00 | 39.4 | 44.0 | -4.6 | 25.5 | 28.1 | -2.5 |
| $6.00 to $7.50 | 27.7 | 26.2 | 1.5 | 25.3 | 27.2 | -1.9 |
| $7.50 to $9.00 | 14.7 | 12.1 | 2.6 | 22.4 | 19.9 | 2.6 |
| $9.00 or more | 12.3 | 10.6 | 1.7 | 21.2 | 19.2 | 2.1 |
| (Average hourly wage in 1995 dollars) | 6.77 | 6.53 | 0.24*** | 7.55 | 7.33 | 0.22*** |
| Weekly Earnings[a] | | | | | | |
| Less than $150 | 13.8 | 16.7 | -2.9***[b] | 10.8 | 12.4 | -1.6***[b] |
| $150 to $225 | 21.8 | 23.8 | -2.0 | 14.4 | 15.4 | -1.0 |
| $225 to $300 | 29.9 | 29.2 | 0.6 | 25.0 | 26.8 | -1.8 |
| $300 to $375 | 16.3 | 14.1 | 2.2 | 21.0 | 18.3 | 2.6 |
| $375 or more | 18.2 | 16.2 | 2.0 | 28.9 | 27.0 | 1.9 |
| (Average weekly earnings in 1995 dollars) | 284.7 | 269.7 | 15.1*** | 326.5 | 314.1 | 12.4*** |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **6,828** | **4,485** | **11,313** |

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimates pertain to those employed in quarter 10 (quarter 16). Because these estimates are conditional on being employed, they are not impact estimates.

[b] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

they differed somewhat across workers. For example, in quarter 16, about one-third earned less than $6.00 per hour, while nearly 20 percent earned more than $9.00 per hour.

Differences in average hourly wage rates between the employed program and control groups were small, but they were positive and statistically significant in *both* periods (that is, the wage differences persisted). Employed program group members earned an average of $0.24 more per hour than employed control group members in their most recent job in quarter 10 ($6.77, compared to $6.53). In quarter 16, the difference in the average wage rate was $0.22 ($7.55, compared to $7.33). Furthermore, a higher percentage of the program group earned higher wages (27 percent earned $7.50 or more per hour in quarter 16, compared to 23 percent of the control group), and a smaller percentage of the program group earned lower wages (31 percent earned less than $6.00 in quarter 16, compared to 34 percent of the control group).[9]

The wage rate gains could be due to several factors. First, as discussed in Glazerman et al. (2000), Job Corps participation leads to statistically significant gains in functional literacy skills. Job Corps raised the average test scores of program group participants at 30 months by about 4 points on the prose literacy scale, 2 points on the document literacy scale, and 5 points on the quantitative literacy scale. In addition, Job Corps moved some participants out of the lowest proficiency level. Thus, increases in the skill level of program participants probably led to increases in labor market productivity and, hence, to higher wages.

The impacts on hourly wages and earnings, however, are larger than can be explained by the impacts on literacy skills alone (Glazerman et al. 2000). Thus, the wage and earnings gains were likely to have also been due to other factors that are influenced by Job Corps but not captured in the test scores. These factors might include impacts on vocational skills for a specific job that are not captured in the literacy test, improvements in social skills and attitudes about work, and credentialing effects from obtaining a GED or vocational certificate. It is also possible that the higher wages of the program group were due to placement assistance they received, which increased their chances of finding a job that matched their skills. However, as reported in Chapter IV, few program participants reported that they received significant placement assistance. Thus, the hourly wage gains were probably due only in small part to the Job Corps placement component.

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[9] We also estimated multivariate models (such as tobit models) to obtain program effects on hourly wage rates. These models controlled for both observable and unobservable differences between the two groups of workers. These results were very similar to the simple program and control group differences.

### 2. Differences in Occupations

The follow-up interviews collected information on the nature of the work performed on each job during the 48-month follow-up period, and the responses were assigned two-digit Standard Occupational Classification (SOC) codes.[10] Occupations were then aggregated into eight broad categories according to two main criteria: (1) each category should correspond to major vocational areas offered in Job Corps, and (2) sample sizes in each category should be large enough to support reasonably precise comparisons between the program and control groups.

Job Corps did not shift workers among the broad occupations in which sample members worked (Table VI.6). Furthermore, the distribution of occupations in which sample members worked changed only slightly over time. About 22 percent of both groups worked in service occupations (such as food and health service) in both quarters. An additional 20 percent worked in construction occupations. About 13 percent worked in sales in quarter 10, compared to about 11 percent in quarter 16. About 11.5 percent in quarter 10 and 13.5 percent in quarter 16 were mechanics, repairers, or machinists. Less than 10 percent were in clerical occupations in quarter 10, but this figure increased to 12.5 percent in quarter 16. Less than 8 percent were in private household

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[10] The responses did not usually contain enough detail to be assigned three-digit SOC codes.

## TABLE VI.6

## OCCUPATIONS AND TYPE OF EMPLOYER ON THE MOST RECENT JOB IN QUARTERS 10 AND 16

(Percentages)

| | Quarter 10 | | | Quarter 16 | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Program Group | Control Group | Difference | Program Group | Control Group | Difference |
| Percent Employed in Quarter | 65.6 | 63.7 | 1.9** | 71.1 | 68.7 | 2.4*** |
| Occupation[a] | | | | | | |
| Services | 24.2 | 21.9 | 2.3 | 21.3 | 20.8 | 0.4**[b] |
| Sales | 12.6 | 14.2 | -1.6 | 9.7 | 12.1 | -2.3 |
| Construction | 20.1 | 20.6 | -0.5 | 20.9 | 20.3 | 0.5 |
| Private household | 6.7 | 7.0 | -0.3 | 6.9 | 7.2 | -0.2 |
| Clerical | 9.5 | 9.3 | 0.1 | 11.8 | 12.8 | -1.0 |
| Mechanics/repairers/ machinists | 12.1 | 11.0 | 1.1 | 13.9 | 13.1 | 0.7 |
| Agriculture/forestry | 2.5 | 3.1 | -0.5 | 2.6 | 2.6 | 0.0 |
| Other | 12.3 | 12.9 | -0.6 | 12.9 | 11.1 | 1.9 |
| Type of Employer[a] | | | | | | |
| Private company | 84.0 | 84.0 | 0.0 | 79.9 | 79.4 | 0.5 |
| Military | 2.1 | 1.9 | 0.2 | 2.6 | 2.0 | 0.5 |
| Federal government | 1.9 | 1.9 | -0.1 | 2.0 | 2.2 | -0.1 |
| State government | 3.9 | 2.9 | 1.0 | 4.2 | 4.7 | -0.5 |
| Local government | 2.5 | 3.1 | -0.7 | 3.0 | 4.0 | -1.0 |
| Self-employed | 4.4 | 5.1 | -0.7 | 5.5 | 5.3 | 0.2 |
| Working without pay in a family business or as a favor | 0.6 | 0.4 | 0.2 | 1.1 | 1.2 | -0.1 |
| Other | 0.7 | 0.7 | 0.0 | 1.6 | 1.2 | 0.5 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **6,828** | **4,485** | **11,313** |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimates pertain to those employed in quarter 10 (quarter 16). Because these estimates are conditional on employment, they are not impact estimates.

[b] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

occupations (such as building and apartment maintenance, babysitting, and child care), or agricultural or forestry trades.

The types of employers that the employed program and control group members worked for were nearly identical. Most youths worked for a private company (84 percent in quarter 10 and 80 percent in quarter 16). Only a small percentage worked for the government (8 percent in quarter 10 and 10 percent in quarter 16), were self-employed (5 percent in both quarters), or were in the military (2 percent in both quarters).

### 3. Differences in Hourly Wages Within Occupations

Similar percentages of the employed program and control group members were in each occupational area. However, the average hourly wage was higher for the employed program group. Thus, there must have been differences between the wages of program and control group members *within* occupations. An important issue is whether these wage gains were concentrated in selected occupations or occurred uniformly across occupations.

In general, the wage gains occurred in most occupation groups (Table VI.7). Employed program members had higher wages in six of the eight occupational areas in quarter 10 and in five of the eight areas in quarter 16, including higher-paying occupations (such as construction) and lower-paying occupations (such as service). Thus, participants probably obtained jobs requiring higher skill levels in most occupational areas.

### 4. Differences in the Availability of Job Benefits

The availability of job benefits is another indicator of job quality. Many, though by no means all, employed control group members were receiving the major fringe benefits in the jobs they held in quarter 10, and benefit receipt rates increased between quarters 10 and 16 as the sample members gained work experience and obtained better jobs (Table VI.8). About 48 percent in quarter 10 and

## TABLE VI.7

## HOURLY WAGES BY OCCUPATION FOR THOSE EMPLOYED IN QUARTERS 10 AND 16

| | Average Hourly Wage in Quarter 10 (in 1995 Dollars) | | | Average Hourly Wage in Quarter 16 (in 1995 Dollars) | | |
|---|---|---|---|---|---|---|
| Occupation | Program Group | Control Group | Difference[a] | Program Group | Control Group | Difference[a] |
| Service | 6.24 | 6.16 | .08 | 6.94 | 6.48 | .46*** |
| Sales | 6.01 | 6.04 | -.02 | 6.73 | 6.44 | .28 |
| Construction | 7.29 | 6.94 | .35** | 8.04 | 7.90 | .14 |
| Private Household | 5.54 | 5.16 | .38 | 6.04 | 6.13 | -.09 |
| Clerical | 7.16 | 6.90 | .26 | 8.27 | 8.06 | .22 |
| Mechanics/Repairers/ Machinists | 7.53 | 6.95 | .58*** | 8.20 | 8.17 | .03 |
| Agriculture/Forestry | 6.55 | 6.89 | -.35 | 6.83 | 6.92 | -.10 |
| Other | 7.44 | 7.10 | .34 | 7.93 | 7.95 | -.03 |
| **Sample Size** | **3,941** | **2,521** | **6,462** | **4,663** | **2,865** | **7,528** |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Because these estimates are conditional on employment, they are not impact estimates.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE VI.8

## BENEFITS AVAILABLE ON THE MOST RECENT JOB IN QUARTERS 10 AND 16 FOR THOSE EMPLOYED

(Percentages)

| | Quarter 10 | | | Quarter 16 | | |
|---|---|---|---|---|---|---|
| Benefits Available[a] | Program Group | Control Group | Difference | Program Group | Control Group | Difference |
| Health Insurance | 50.5 | 48.3 | 2.2* | 57.4 | 54.3 | 3.0** |
| Paid Sick Leave | 41.7 | 38.4 | 3.3*** | 47.3 | 44.5 | 2.8** |
| Paid Vacation | 56.1 | 54.2 | 1.9 | 62.9 | 60.7 | 2.2* |
| Child Care Assistance | 14.8 | 12.6 | 2.1** | 15.8 | 14.2 | 1.6* |
| Flexible Hours | 55.0 | 53.1 | 1.9 | 57.4 | 56.7 | 0.6 |
| Employer-Provided Transportation | 19.1 | 18.0 | 1.1 | 19.5 | 18.7 | 0.8 |
| Retirement or Pension Benefits | 41.6 | 38.0 | 3.6*** | 48.3 | 43.7 | 4.6*** |
| Dental Plan | 42.8 | 39.2 | 3.6*** | 49.9 | 47.3 | 2.5** |
| Tuition Reimbursement or Training Course | 25.4 | 22.2 | 3.2*** | 28.6 | 26.4 | 2.1** |
| **Sample Size** | **3,941** | **2,521** | **6,462** | **4,663** | **2,865** | **7,528** |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimates pertain to those employed in quarter 10 (quarter 16). Because these estimates are conditional on employment, they are not impact estimates.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

54 percent in quarter 16 received health insurance, about 54 percent in quarter 10 and 61 percent in quarter 16 had paid vacation, and 38 percent in quarter 10 and about 44 percent in quarter 16 had retirement or pension benefits.

Job Corps appears to have had small positive effects on the availability of benefits on the job. Employed program group members were more likely to have each type of benefit available than were employed control group members, and the differences were similar in quarters 10 and 16. The differences were small, though many are statistically significant. For example, in quarter 16, about 57 percent of the program group received health insurance compared to 54 percent of the control group (a statistically significant increase of 3 percentage points, or nearly 6 percent). These findings provide additional evidence that Job Corps participants obtained better jobs as a result of their gains in skill level.

As described more fully in McConnell et al. (2001), the impacts on total compensation were somewhat larger than the impacts on earnings, because employed program group members were more likely to receive fringe benefits than employed control group members.

## C. IMPACTS ON PARTICIPATION IN ANY ACTIVITY

Both current employment and current education and training are likely to improve youths' long-run employment prospects. Each of these activities provides skills and experiences that employers value. In this section, we examine the extent to which eligible Job Corps applicants engaged in either or both of these activities.

Chapter V showed that program group members were more likely than control group members to participate in education and training programs during the first two years after random assignment. The impacts were largest in the early part of the follow-up period, when most program group members were enrolled in Job Corps, decreased as participants left Job Corps, and were very small after quarter 8. Conversely, control group members worked more than program group members during the early part of the follow-up period, and impacts on employment did not become positive until quarter 8. To assess the extent to which these opposing impact trends offset each other, we calculated program impacts on being either employed or in an education or training program, by quarter and over the entire 48-month period.

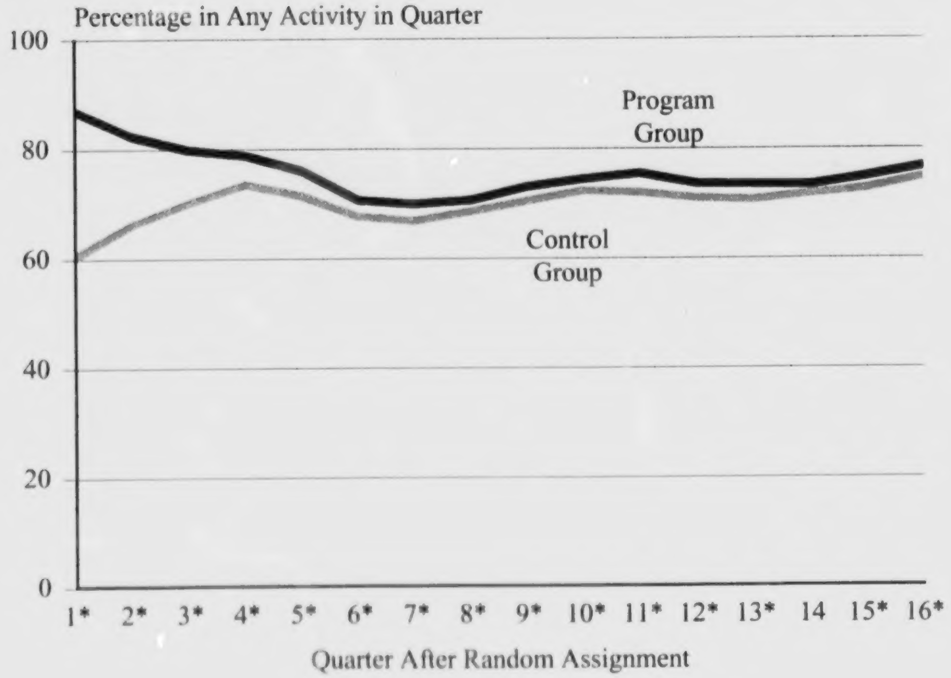
Many control group members worked or engaged in education or training during each quarter of the follow-up period (Figure VI.4 and Table VI.9). The percentage of the control group in an activity increased during the first year after random assignment (from 60 percent in quarter 1 to 74 percent in quarter 4) because both employment and school enrollment rates increased. The percentage remained relatively constant after the first year (it was 72 percent in quarter 10 and 75 percent in quarter 16), because increases in the employment rate offset declines in enrollment in school. Nearly all control group members either worked or undertook education or training at some point during the 48-month period. Since all these youths had made the decision to apply to Job Corps, this high level of productive activity is not surprising.

Estimated impacts on working or being in school were positive and statistically significant in each quarter of the follow-up period. The impacts were largest during the first year after random assignment, because most program group members were enrolled in Job Corps then. The program group's higher rates of participation in education or training during this period more than offset the higher employment rates of the control group.

The impacts were positive, but they were much smaller between quarters 4 and 7, because impacts on participation in education and training programs decreased as more program group members left Job Corps and because the declines in education were not fully offset by increases in employment. Impacts in the second half of the follow-up period (quarters 8 to 16) remained positive

## FIGURE VI.4

## PERCENTAGE EMPLOYED OR IN SCHOOL, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

TABLE VI.9

IMPACTS ON BEING EMPLOYED OR IN AN EDUCATION OR TRAINING PROGRAM

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage in Any Activity, by Quarter After Random Assignment | | | | | | |
| 1 | 87.0 | 60.4 | 26.6*** | 96.6 | 37.0*** | 62.1 |
| 2 | 82.4 | 66.2 | 16.2*** | 89.5 | 22.6*** | 33.7 |
| 3 | 79.8 | 70.2 | 9.7*** | 84.4 | 13.4*** | 18.9 |
| 4 | 78.8 | 73.6 | 5.2*** | 81.3 | 7.2*** | 9.8 |
| 5 | 76.0 | 71.5 | 4.5*** | 78.0 | 6.3*** | 8.8 |
| 6 | 70.7 | 67.7 | 3.0*** | 71.7 | 4.2*** | 6.2 |
| 7 | 70.0 | 66.9 | 3.1*** | 70.6 | 4.3*** | 6.5 |
| 8 | 70.7 | 68.6 | 2.1** | 71.3 | 2.9** | 4.2 |
| 9 | 73.1 | 70.5 | 2.6*** | 74.0 | 3.7*** | 5.2 |
| 10 | 74.5 | 72.4 | 2.1** | 75.3 | 2.9** | 4.0 |
| 11 | 75.4 | 72.0 | 3.4*** | 76.0 | 4.7*** | 6.6 |
| 12 | 73.6 | 71.0 | 2.6*** | 73.7 | 3.6*** | 5.2 |
| 13 | 73.5 | 70.7 | 2.8*** | 73.7 | 3.9*** | 5.6 |
| 14 | 73.4 | 71.8 | 1.6* | 73.4 | 2.2* | 3.1 |
| 15 | 74.9 | 72.7 | 2.2** | 75.6 | 3.0** | 4.2 |
| 16 | 76.7 | 74.8 | 1.9** | 77.0 | 2.7** | 3.6 |
| Percentage in Any Activity at 48 Months | 67.6 | 65.2 | 2.4*** | 67.9 | 3.3*** | 5.2 |
| Percentage Ever in an Activity | 99.6 | 98.2 | 1.4*** | 100.0 | 2.0*** | 2.0 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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(though small), because employment rates of the program group were higher. The impact per participant in quarter 16 was 2.7 percentage points, a 3.6 percent gain due to Job Corps participation.

Impacts on the proportion of weeks and hours per week spent working or in an education or training program follow the same pattern (Tables D.1 and D.2). They were positive and statistically significant in all quarters, but largest early in the follow-up period, when most program group members were enrolled in the program. In sum, Job Corps had a sustained positive effect on promoting activities aimed at improving participants' long-run employment prospects.

## D. FINDINGS FOR SUBGROUPS

Overall, Job Corps produced modest gains in employment and earnings starting about two years after youths applied for the program and were determined eligible. Positive impacts for the full sample, however, could mask important differences in program impacts across subgroups of students. An important question is whether these positive impacts were similar for most subgroups of students or were concentrated among certain groups. This section provides evidence on this question.

After briefly summarizing the subgroup findings, we present detailed findings for the most important subgroups--those defined by age, gender, and residential or nonresidential assignment. We present the full detail on employment and earnings impacts for these groups. In the third section, we discuss findings for other subgroups of interest--whether the youth had a high school diploma or GED at baseline, whether the youth was ever arrested before application, race and ethnicity, and whether the youth applied to Job Corps before or after the new ZT policies became effective. For these subgroups, the discussion focuses on employment and earnings in year 4.

For each subgroup, we present impacts per eligible applicant and impacts per program participant. However, it is especially important to focus on the impacts per participant in the subgroup analysis. Rates of Job Corps enrollment among the program group differed somewhat across the subgroups (as discussed in Chapter IV). Consequently, the impacts per eligible applicant were inflated by different participation rates in calculating the impacts per participant. Because of these differing participation rates across subgroups, impacts per participant provide the most accurate picture of relative program impacts across the different groups.

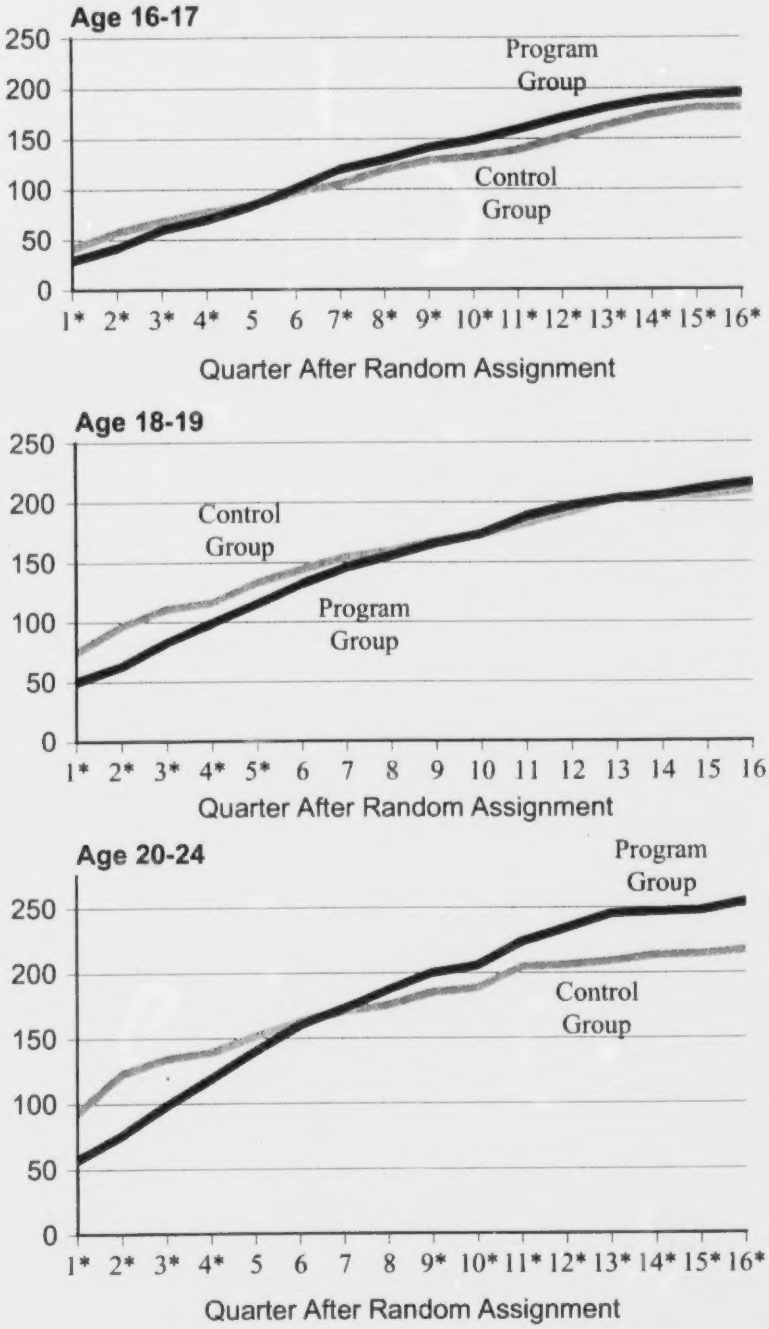
### 1. Impacts by Age

As one would expect, employment rates and average earnings of older applicants were higher than those of younger applicants during each quarter of the 48-month follow-up period (Figure VI.5 and Tables D.3 to D.5). Among the control group, employment and earnings increased over time for all age groups but increased proportionately more for those 16 and 17 years old. For example, average earnings per week of 16- and 17-year-old control group members nearly tripled, from $61 in year 1 to $175 in year 4, whereas those of control group members 20 and older less than doubled during the same period (from $123 to $214).

The impacts on employment and earnings were large for those who were 20 or older at application to Job Corps (Figures VI.5 and VI.6 and Tables D.3 to D.5). Impacts on their earnings per week became positive in quarter 7 and were statistically significant by quarter 9. The impacts increased throughout the postprogram period; the impact per eligible applicant more than doubled from $15 in quarter 9 to $37 in quarter 16. In year 4, the impact on earnings per participant was about $50 per week (or $2,600 in total)--a 25 percent gain. Impacts per participant on the quarterly employment rates and the percentage of weeks employed in year 4 were about 8 percentage points each and are statistically significant. Over the entire 48-month period, participants earned about $11

FIGURE VI.5

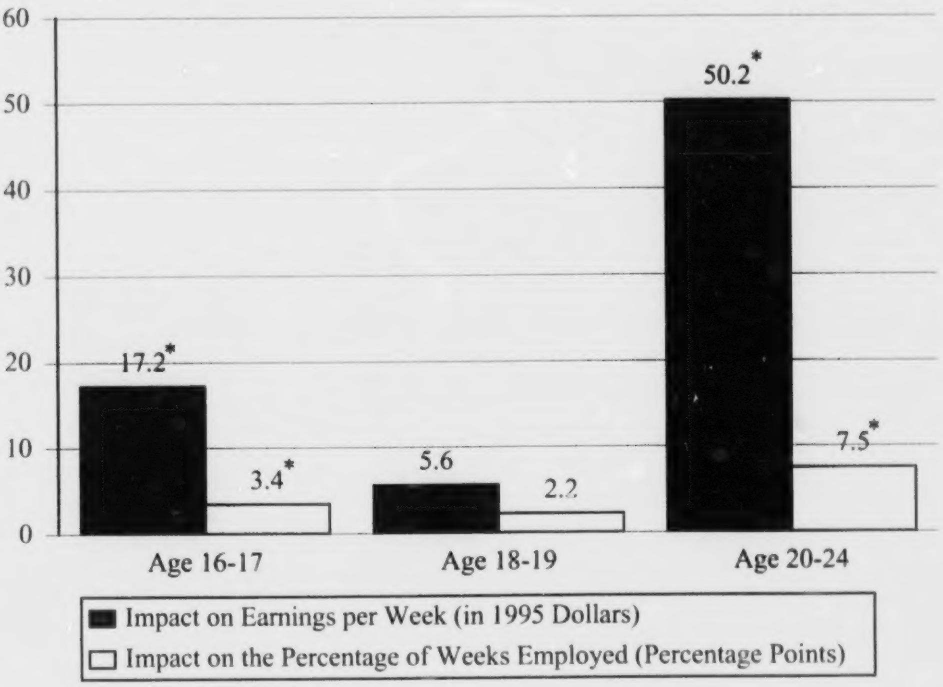
AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS), BY QUARTER AND AGE



*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## FIGURE VI.6

## IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY AGE



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Estimated impact per participant is statistically significant at the 5 percent level.

more per week (about $2,300 in total) more than they would have if they had not enrolled in Job Corps.[11]

The program also produced meaningful earnings gains for 16- and 17-year-olds. Impacts on earnings per week were positive beginning in quarter 6, and were statistically significant beginning in quarter 7. The earnings impacts remained relatively constant between quarters 7 and 16. In year 4, the impact per participant on earnings was $17 per week (nearly $900 in total)--a 10 percent gain. Job Corps participation also increased the percentage of weeks employed and average hours per week employed in year 4 for this group by about 7 percent, and these impacts are statistically significant. The impact per participant on earnings over the entire 48-month period was about $1,800.

The employment and earnings impacts were small for 18- and 19-year-old participants. In year 4, the impact per participant on earnings per week was about $6 and the impact on the percentage of weeks employed was about 2 percentage points. These small positive impacts, however, are not statistically significant. Furthermore, the small impacts for those 18 and 19 were found across other subgroups (such as gender and education level at baseline).

The results for the 18- and 19-year-olds are puzzling in light of the positive impacts found for the other age groups. The baseline characteristics of those 18 and 19 are not unusual (Schochet 1998a). In addition, the Job Corps experiences of 18- and 19-year-old participants appear to have been similar to those of participants in other age groups (as discussed in Chapter IV). Furthermore, the estimated impacts on education-related outcomes were large for all age groups (as discussed in Chapter V). Finally, the small impacts for those 18 and 19 appear to be due to high employment and earnings levels for the control group and not to low levels for the program group.[12] Thus, it is difficult to determine whether impact findings for this group are anomalous.

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[11] We also estimated impacts for each age group separately (that is, for those 20, 21, 22, 23, and 24) and found very similar results for each age group.

It is noteworthy that the differences in earnings impacts by age were not due to differences in school enrollment rates by age. About 17 percent of program and control group members in each age group were enrolled in an education program per quarter in year 4.

### 2. Impacts by Gender

Impacts on employment and earnings were very similar for males and females (Figures VI.7 and VI.8 and Tables D.6 and D.7). Indeed, the timing of the overtaking points and the size of the impacts were similar. For example, the impact per participant on year 4 earnings per week was $24 for males (an 11 percent increase) and $21 for females (a 14 percent increase). Impacts on hours worked and hourly earnings were also very similar for males and females. The differences between the year 4 impact estimates by gender are not statistically significant. The gender findings are similar across most other subgroups.

The finding that Job Corps improved employment-related outcomes for both males and females is of policy importance because of differences in the characteristics and programmatic needs of these groups. Female students tend to be older, to have completed high school, to have children, and to be nonresidential students. Thus, the program effectively serves these two groups of students with different training needs and barriers to successful employment.

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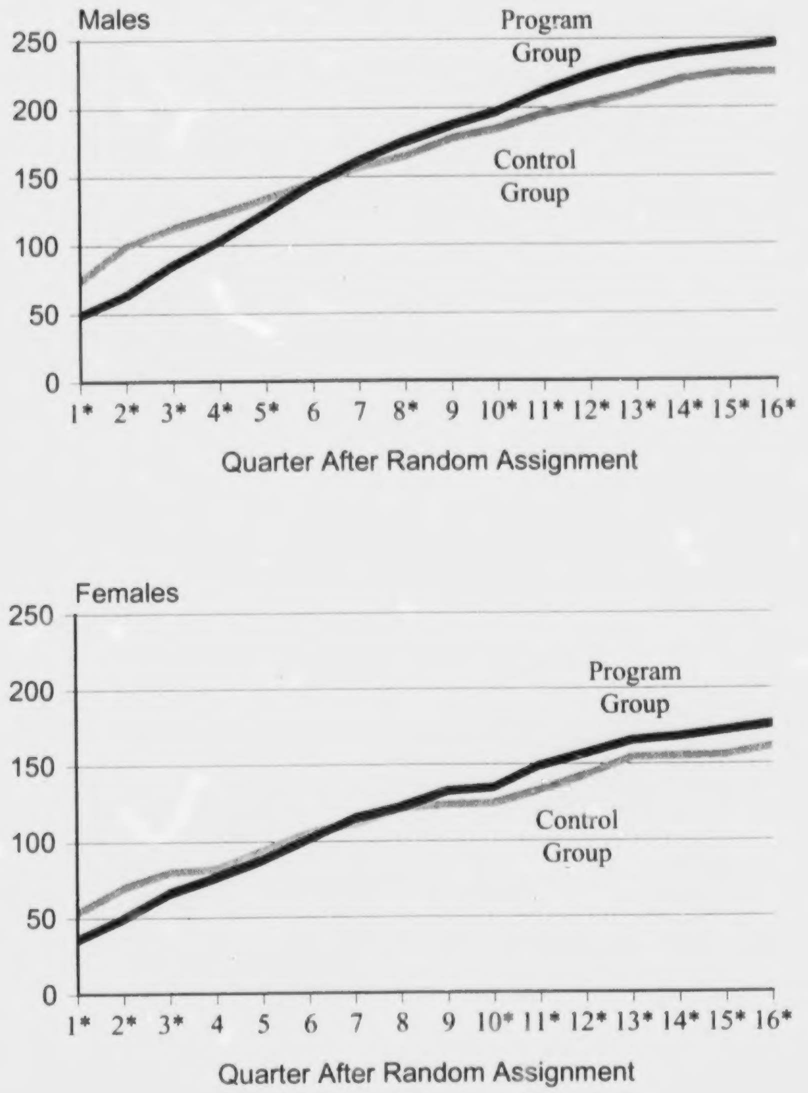
[12] For example, among the control group, average weekly earnings in year 4 of those 18 and 19 were 18 percent higher than the average weekly earnings of those 16 and 17, but were only 4 percent less than the average weekly earnings of those 20 to 24. The corresponding figures for the program group were 12 percent and 15 percent, respectively. Thus, the average earnings differences between those 18 and 19 and those 20 to 24 in the control group were much less than one would have anticipated.

FIGURE VI.7

AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS), BY QUARTER AND GENDER

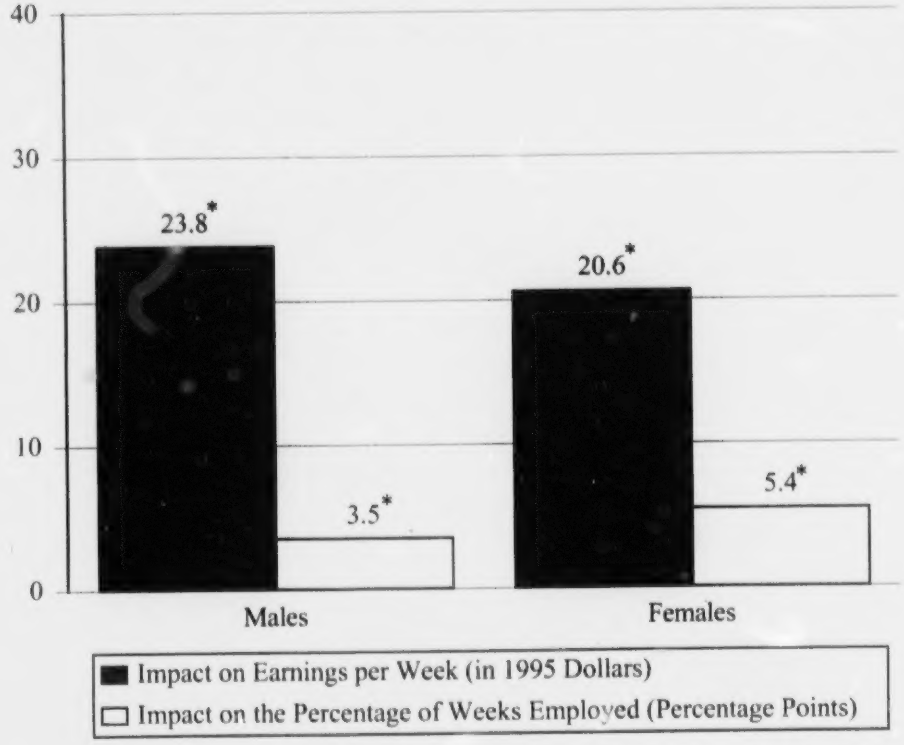


Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

FIGURE VI.8

## IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY GENDER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Estimated impact per participant is statistically significant at the 5 percent level.

### 3. Impacts for Residential and Nonresidential Students

Most students reside at their center while attending Job Corps. Indeed, one eligibility criterion is that the student must live in a home or community environment so debilitating that the youth cannot benefit from education and job training while living at home. Yet up to 20 percent of Job Corps slots can be used to serve nonresidential students--those who live at home while attending Job Corps. About 12 percent of students were nonresidential during the period of the study. Nonresidential students must live within commuting distance of their center, and they must be judged able to benefit from Job Corps without leaving their community.

Impacts of the residential component were estimated by comparing the outcomes of program group members designated for a residential slot before random assignment with the outcomes of control group members designated for a residential slot. Similarly, the impacts of the nonresidential component were estimated by comparing the experiences of program and control group members designated for nonresidential slots. Accordingly, the analysis examines (1) the effectiveness of the residential program for youths who are typically assigned to residential slots, and (2) the effectiveness of the nonresidential program for youths who are typically assigned to nonresidential slots. Differences in the students assigned to each component require that we interpret the findings cautiously: they do *not* tell us about the effectiveness of each component for the average Job Corps student or how students assigned to one component would have fared in the other.

These important qualifications can be understood further by noting that the characteristics of residential and nonresidential designees differ in important ways. As described in Chapter III, for both males and females, nonresidential designees are much more likely than residential designees to be older, to have children, and to have a high school credential, and are less likely to ever have been arrested. Thus, the residential and nonresidential program components serve very different students, and our design can only address the extent to which each component effectively serves students suited for it.

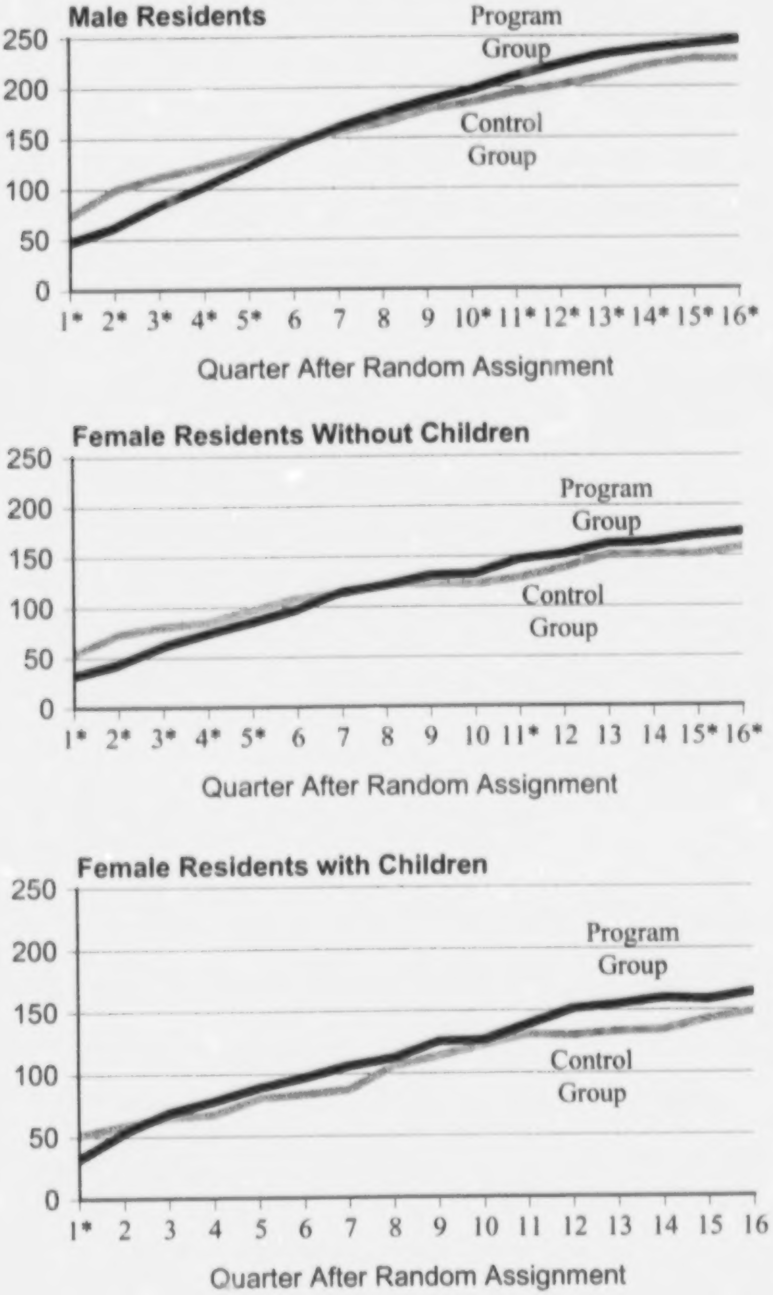
For each component, we present separate impact estimates for (1) males, (2) females without children, and (3) females with children. Samples for some of these subgroups are small (for example, the control group contains only about 200 female residential designees with children, 200 female nonresidential designees without children, and 200 male nonresidential designees). Accordingly, some of the subgroup impact estimates are imprecise. Still, the differences in students served in each component made it important to present separate estimates for these groups.

### a. Impacts for Residential Students

Job Corps was effective for students assigned to the residential program and similarly effective for broad groups of students (Figures VI.9 and VI.10 and Tables D.8 to D.10). The estimated impacts on employment and earnings in years 3 and 4 were very similar for male residents, female residents with children, and female residents without children. The impact per participant on year 4 earnings per week was about $21 for males and for females without children, and it was $31 for females with children. These impacts translate into percentage increases in earnings of 10 percent for males, 15 percent for females without children, and 21 percent for females with children. These results suggest that disadvantaged youths who are suitable for the residential component can benefit from being removed from their home environments and given intensive services in a residential setting for a significant period of time.

FIGURE VI.9

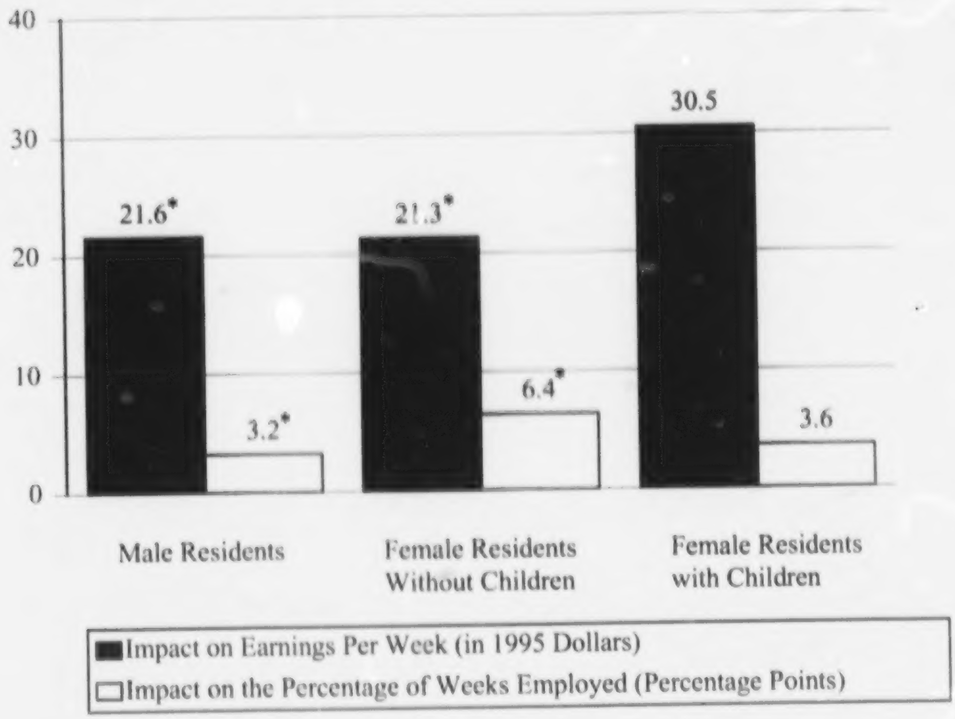
AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS) FOR RESIDENTIAL DESIGNEES, BY QUARTER AND GENDER



*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

FIGURE VI.10

IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4 FOR RESIDENTIAL DESIGNEES, BY GENDER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Estimated impact per participant is statistically significant at the 5 percent level.

### b. Impacts for Nonresidential Students

The nonresidential component was also effective overall and for most students that it served. The nonresidential component substantially improved the employment-related outcomes of females with children and males, but it did not improve these outcomes for females without children (Figures VI.11 and VI.12 and Tables D.11 to D.13). Participation in the nonresidential component improved earnings per week in year 4 by more than $35 for females with children (an increase of 24 percent), and by more than $55 for males (an increase of 26 percent).[13] The estimated impacts on earnings for females without children are not statistically significant.

The finding that estimated program impacts were large for females with children is important because, as discussed, their barriers to successful employment are particularly acute. For example, these women (who represent about 30 percent of all female students and about half of all nonresidential students) tend to be highly dependent on public assistance, and many lack adequate support systems. Thus, the fact that Job Corps can increase employment and earnings for this group is an important policy finding.

### c. Interpretation of Findings

The impact findings by residential status should be interpreted with caution. As discussed, our estimates provide information about the effectiveness of each component for the populations it serves. The estimates cannot be used to assess how a youth in one component would fare in the other one, or how effective each component would be for the average Job Corps student. This is because the characteristics of residents differ from those of nonresidents in ways that can affect outcomes.

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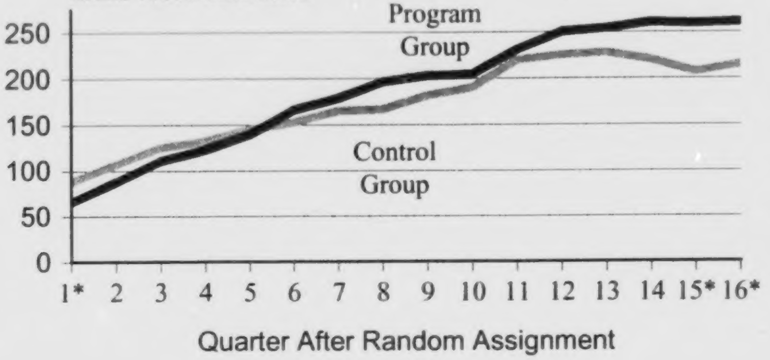
[13] The large earnings impact for males was due in part to an anomalous dip in the average earnings of control group members in this group during year 4. Thus, while we believe that the impact for this group is positive, our estimated impact may be overstated.

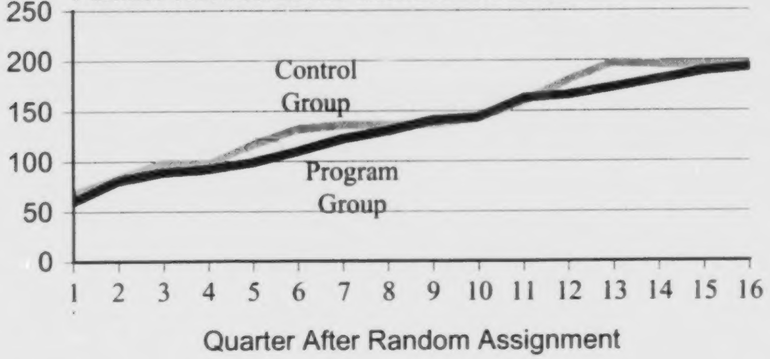
FIGURE VI.11

AVERAGE EARNINGS PER WEEK (IN 1995 DOLLARS) FOR NONRESIDENTIAL DESIGNEES, BY QUARTER AND GENDER

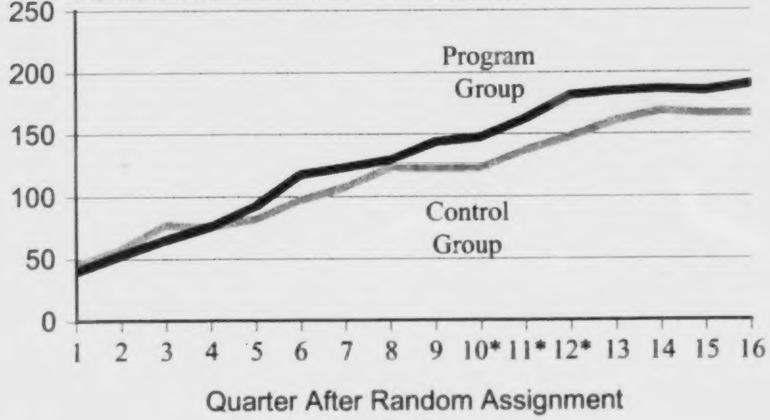
**Male Nonresidents**



**Female Nonresidents without Children**



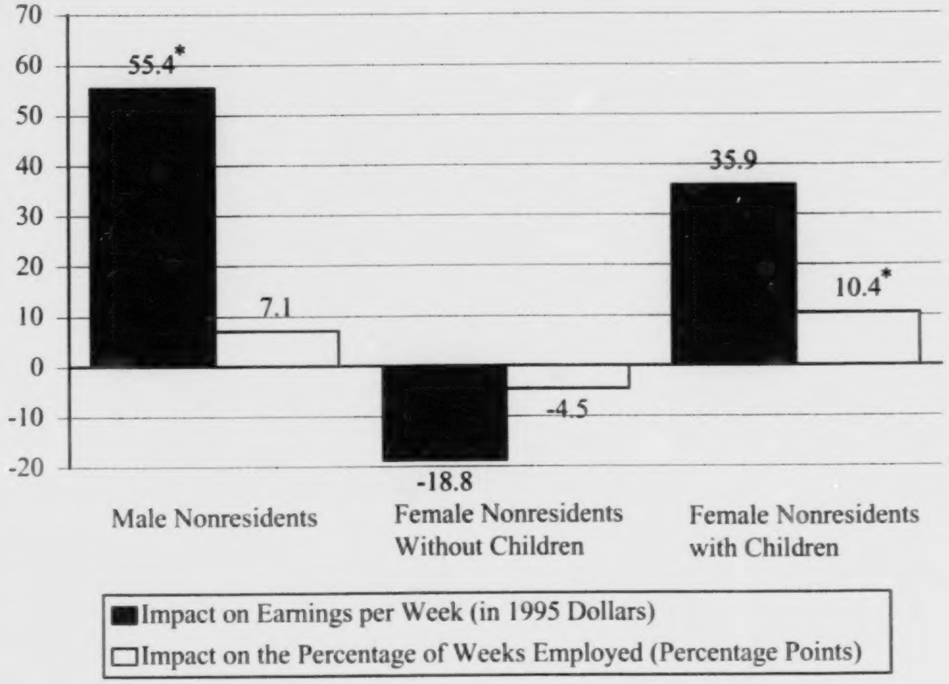
**Female Nonresidents with Children**



*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

FIGURE VI.12

IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4 FOR NONRESIDENTIAL DESIGNEES, BY GENDER



Source: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

*Estimated impact per participant is statistically significant at the 5 percent level.

For example, we find positive impact for males in the residential component and for males in the nonresidential component. It is tempting, then, to conclude that all males should receive training in the slightly less expensive nonresidential component.[14] However, our results *cannot* be used to support this conjecture, because there are known differences in the characteristics of male residents and male nonresidents. While it is possible to control for some of these differences (such as age, education level, and the presence of children), others (such as family commitments and support, and motivation) are probably correlated with outcomes and cannot be measured. These unmeasured differences could lead to erroneous conclusions about how residential males would fare in the nonresidential component (and vice versa).

Furthermore, most centers with nonresidential slots also have residential slots. Thus, nearly all nonresidential students train with residential students and may benefit from this interaction. It would be impossible from our results to determine the effectiveness of the nonresidential component if nonresidential and residential students enrolled in separate centers.

In sum, our results shed light on how well the residential program serves youths who are suitable for the residential component, and how well the nonresidential program serves youths who are suitable for the nonresidential component, given the interaction of students in the two components.

### 4. Impacts for Other Key Subgroups

Positive impacts on postprogram employment and earnings were found for most other key subgroups defined by youth characteristics. Beneficial impacts were found both for those who lacked a high school credential at application and for those with a high school credential, although impacts were particularly large for those 20 and older with a high school credential. Whites and

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[14] As discussed in McConnell et al. (2001), the cost per participant is about 16 percent less for nonresidential students than for residential students.

African Americans experienced earnings gains, although no gains were found for Hispanics. Although some evidence suggests that earnings impacts were smaller for those with serious arrest charges, impacts were similar for those who had and had not been arrested. Impacts were the same for those who applied before and after the new Job Corps ZT policies took effect.

### a. Educational Attainment

Impacts on employment and earnings were positive and statistically significant for those with a high school credential (GED or high school diploma) *and* for those who lacked a high school credential at random assignment (Figure VI.13 and Table D.14). Across all ages, participants with a high school credential earned an average of about $33 more per week in year 4 than they would have if they had not enrolled in Job Corps, and their percentage of weeks worked in year 4 was about 5 percentage points higher. Similarly, the impact per participant on year 4 earnings per week for those without a high school credential at baseline was about $19, and the impact on the percentage of weeks worked was 4 percentage points. The differences between the impacts for those with and without a high school credential are not statistically significant.[15]

The estimates for students without a high school credential are heavily influenced by the 16- and 17-year-old students, nearly all of whom had no credential. In contrast, about half the students 20

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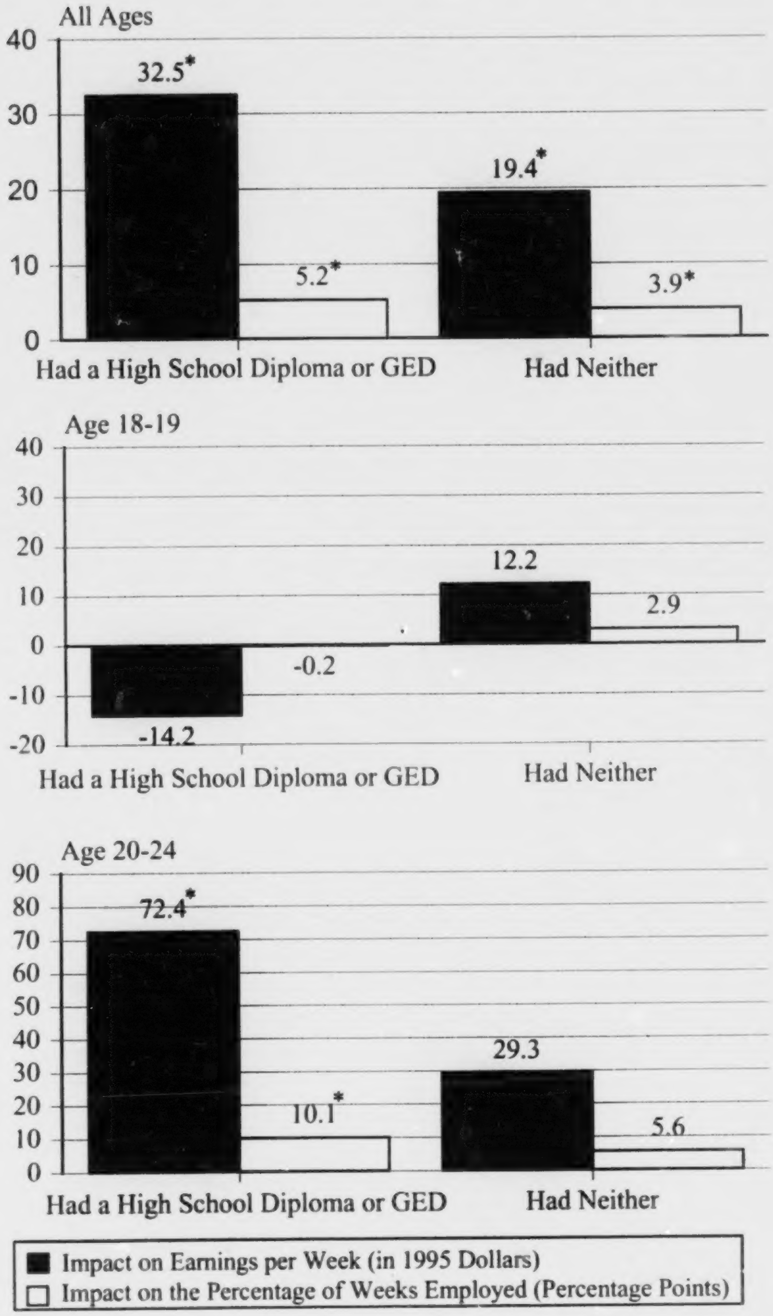
[15] We also estimated separate impacts for those with a GED and those with a high school diploma at random assignment. The employment and earnings *levels* for those with a GED and those with a high school diploma were similar, although the *impacts* for those with a GED and those who lacked a high school credential were similar. The estimated impacts for those with a GED are not statistically significant. Furthermore, sample sizes are small for the GED group (see Table A.1). Thus, we are not confident that the GED results represent true effects; hence, we do not highlight them.

FIGURE VI.13

IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY HIGH SCHOOL CREDENTIAL STATUS AND AGE



*Estimated impact per participant is statistically significant at the 5 percent level.

or older had no credential. To disentangle the effects of age and educational attainment, we also estimated impacts by high school credential status for the older age groups separately (Figure VI.13).

Among those 20 to 24, impacts were positive for those both with and without a high school credential, although they were much larger for those with one. The impact per participant on earnings per week in year 4 was more than $72 for those with a credential, which translates to a 36 percent increase due to program participation. The corresponding impact for 20- to 24-year-olds with a GED or high school diploma was about $29. The estimated impacts for the 18- and 19-year-olds are not statistically significant for those either with or without a high school credential, although the estimates were larger for those without one.

### b. Arrest Experience

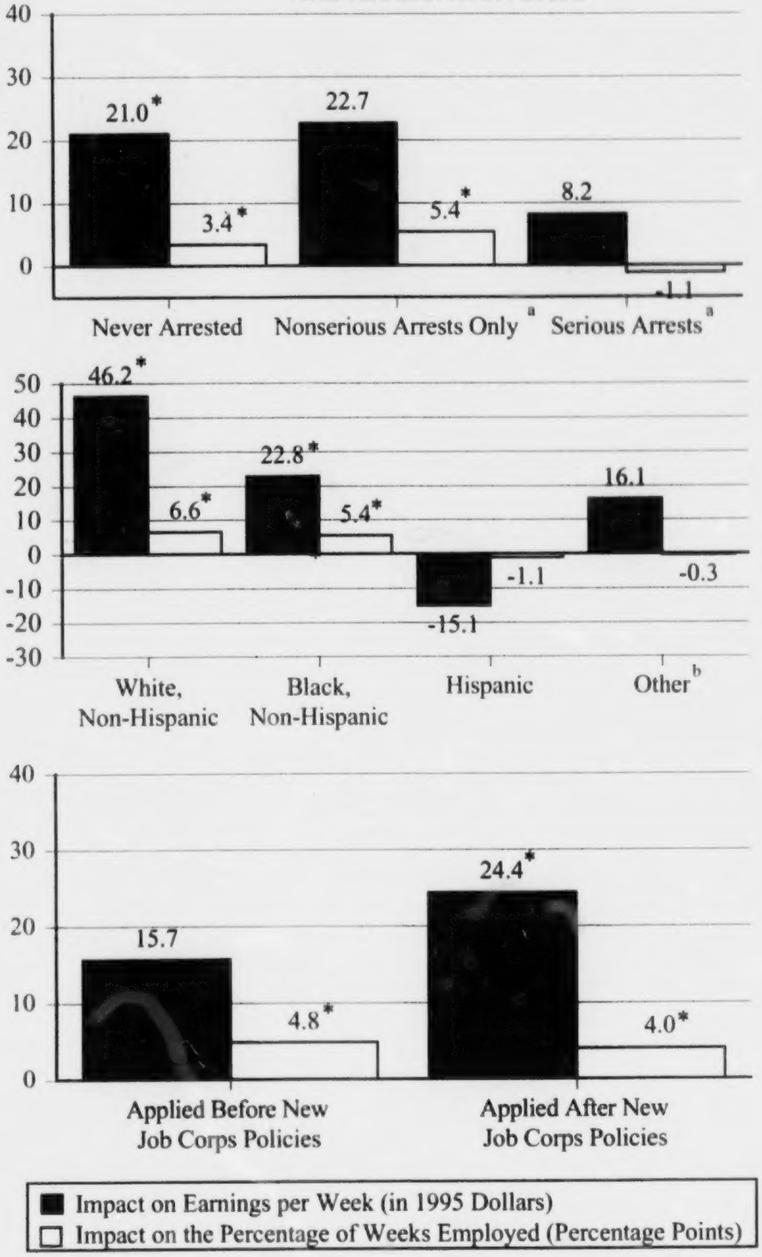
To be eligible for Job Corps, applicants must be free of behavioral problems that would prevent them from adjusting to Job Corps' standards of conduct or that would pose risks to other students. While prior involvement with the criminal justice system does not disqualify an applicant, youths with such involvement are carefully screened by the OA agency and often by the regional office. An important policy question is whether Job Corps can effectively serve those who have had problems with the law.

Job Corps impacts on employment and earnings were similar for those who were never arrested and those who were arrested for nonserious crimes (Figure VI.14 and Table D.14). The impact estimate on earnings per week in year 4 was about $22 for program participants in both groups.

The estimated impacts for those who were ever arrested for serious crimes (murder, aggravated assault, robbery, and burglary), however, were smaller. These results suggest that those who have had serious encounters with the law do not benefit significantly from participation in Job Corps.

FIGURE VI.14

IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK AND THE PERCENTAGE OF WEEKS EMPLOYED IN YEAR 4, BY ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE



[a] Serious arrest charges are murder, assault, robbery, and burglary.

[b] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Estimated impact per participant is statistically significant at the 5 percent level.

However, the group with serious arrests is very small (less than 5 percent of the sample). Thus, conclusions for this group should be treated with caution.

### c. Race and Ethnicity

Job Corps was more effective for whites and African Americans than for Hispanics and other racial and ethnic groups (which includes American Indians, Alaskan Natives, Asians, and Pacific Islanders). As shown in Figure VI.14 and Table D.14, the estimated impact on year 4 earnings per week was $46 for white students and $23 for African American students, and both are statistically significant. The percentage increase in earnings was 21 percent for whites and 14 percent for African Americans. We find no program impacts for Hispanics. In addition, the impact estimates were small and not statistically significant for the remaining racial and ethnic group. The differences between the year 4 earnings impacts across the four racial and ethnic groups are statistically significant.

The finding of no program effects for Hispanics (who are about 18 percent of all youths served by Job Corps) is puzzling because they cannot be explained by differences in program group participation in education and training programs by race and ethnicity. The Job Corps enrollment rate among the program group was similar for Hispanics and other racial and ethnic groups, and the average duration of stay in Job Corps was actually *longer* for Hispanics (9.4 months, compared to 7.7 months). Furthermore, our process analysis site visits to Job Corps centers revealed no differences in the quality of Job Corps services provided to Hispanics and other youths. Finally, the impact on hours spent in all education and training programs during the four-year follow-up period was *larger* for Hispanics than for the other racial and ethnic groups (about 1,200 hours, compared to about 975 hours).

We conducted several additional analyses to help explain the impact findings for Hispanics. First, we estimated program impacts by race and ethnicity across other key subgroups defined by gender, age, and educational level, and found that the impacts for Hispanics were small across each of these subgroups (Table D.15). For example, estimated impacts for earnings in year 4 were not statistically significant for Hispanic males, females, 16- and 17-year-olds, or 20- to 24-year-olds, whereas earnings impacts were positive for whites and African Americans in each of the gender and age groups.

Second, we compared key baseline characteristics of Hispanics, whites, and African Americans in our sample (Table VI.10). Potential differences in the characteristics of Hispanics and other youths could account for the impact findings if Hispanics are more likely to have characteristics associated with smaller impacts.

The main observable differences between Hispanics and other racial and ethnic groups are their geographic locations and primary languages (Table VI.10). Hispanics are heavily concentrated in regions 2, 6, and 9; more than 60 percent of Hispanics live in these three regions, as compared to about 20 percent of whites and African Americans. English is the primary language for less than one-half of Hispanics but for nearly all whites and African Americans. Furthermore, OA counselors deemed that about 12 percent of Hispanics needed a bilingual program in Job Corps, as compared to less than 1 percent of whites and African Americans. Interestingly, however, the age and gender distributions, education levels, and employment, welfare, and arrest histories prior to application are very similar for Hispanics and African Americans.

On the basis of these findings, we estimated impacts for Hispanics, whites, and African Americans by (1) region, (2) whether English was the youth's primary language, and (3) whether

## TABLE VI.10

## KEY BASELINE CHARACTERISTICS, BY RACE AND ETHNICITY
(Percentages)

| Baseline Characteristic | White, Non-Hispanic | Black, Non-Hispanic | Hispanic |
|---|---|---|---|
| **Demographic Characteristics** | | | |
| Age at Application | | | |
| 16 to 17 | 41.4 | 43.0 | 40.1 |
| 18 to 19 | 32.1 | 31.5 | 32.3 |
| 20 to 24 | 26.6 | 25.5 | 27.6 |
| Female | 33.3 | 44.3 | 43.3 |
| Region | | | |
| 1 | 8.3 | 2.3 | 6.3 |
| 2 | 2.5 | 5.7 | 8.4 |
| 3 | 10.1 | 18.9 | 6.5 |
| 4 | 16.2 | 35.3 | 11.1 |
| 5 | 9.8 | 13.3 | 4.0 |
| 6 | 12.0 | 11.8 | 29.1 |
| 7/8 | 21.7 | 8.3 | 8.2 |
| 9 | 5.6 | 3.8 | 22.7 |
| 10 | 13.9 | 0.7 | 3.8 |
| Native Language Is English | 98.8 | 97.8 | 46.2 |
| Had Children (for Females) | 18.4 | 35.8 | 26.3 |
| Had a High School Diploma or GED | 27.8 | 20.8 | 22.2 |
| Received Welfare in the Past Year[a] | 47.5 | 68.3 | 60.8 |
| Had a Job in the Past Year | 74.6 | 59.3 | 62.5 |
| Was Ever Arrested | 28.1 | 22.0 | 20.8 |

TABLE VI.10 *(continued)*

| Baseline Characteristic | White, Non-Hispanic | Black, Non-Hispanic | Hispanic |
|---|---|---|---|
| **Anticipated Program Enrollment Information** | | | |
| Needs a Bilingual Program in Job Corps | 0.9 | 0.7 | 11.7 |
| Designated for a Residential Slot | 92.3 | 82.4 | 84.9 |
| Designated for a CCC | 29.2 | 8.9 | 9.0 |
| **Sample Size** | **2,982** | **5,541** | **1,961** |

SOURCE: Baseline Interview data and ETA-652 data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

[a] Welfare receipt includes AFDC/TANF, food stamps, and other public assistance.

the youth needed a bilingual program in Job Corps. In addition, we estimated impacts by race/ethnicity and by whether the youth was designated for one of 23 centers where at least 25 percent of students were Hispanic.[16,17] We conducted this analysis to test the hypothesis that impacts for Hispanics were small because impacts for subgroups in which Hispanics were heavily concentrated were small.

We strongly rejected this hypothesis, however, because estimated impacts for Hispanics were small across *all* levels of the tested subgroups (Table D.15). For example, the impacts for Hispanics were not statistically significant for those in regions and centers in which Hispanics were heavily concentrated *or* for those in other regions and centers with lower concentrations of Hispanic students. Furthermore, impact estimates for whites and African Americans were mostly positive in areas with large concentrations of Hispanic students (although they were larger in other areas). Similarly, impacts did not differ for Hispanics whose primary language was English or for those whose primary language was Spanish.

These findings support our conclusion that Job Corps did not appear to improve the postprogram employment-related outcomes of Hispanic students. Although Hispanic students in the program group were successful in Job Corps, their in-program success did not translate into postprogram earnings gains. This finding, pervasive among Hispanic students, is due neither to their personal characteristics (such as age, gender, or English language status) nor to the centers or regions of the country in which they typically enroll.

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[16] As part of the application process, OA counselors provided information on the center to which a youth was likely to be assigned on the Supplemental ETA-652 form. This information was collected prior to random assignment, and thus, is available for both the program and control groups. Impacts for groups of centers were obtained by comparing the outcomes of program group and control group members who were designated for those centers.

[17] Of the 23 largely Hispanic centers, 8 were in region 9; 5 were in region 6; 5 were in region 2; 2 were in region 1; and 1 each was in regions 4, 7/8, and 10.

### d. Job Corps Application Date and the New Job Corps Policies

Job Corps instituted strict ZT policies for violence and drugs in March 1995 (early in the sample intake period for the study) in response to congressional concerns about safety at centers. Students suspected of specific acts of violence or of possession or sale of illegal drugs are now removed from the center immediately and, if fact-finding establishes that they committed the alleged offenses, they are terminated from the program. To assess the extent to which these new policies might have affected the impact estimates, we calculated impacts separately for those who applied before and after March 1, 1995.

Postprogram employment and earnings impacts were similar for the cohorts enrolled before and after the ZT policies took effect (Figure VI.14 and Table D.14). The impact estimate on earnings per week in year 4 was about $24 for the post-ZT group, compared to $16 for the pre- ZT group, and the difference in the impact estimates is not statistically significant. In addition, Job Corps enrollment rates among the program group, the distribution of the duration of stay in the program, and impacts on education-related outcomes were similar for the two groups. Thus, it does not appear that the new policies had much effect on earnings impacts.

The impact estimates for the pre-ZT group should be interpreted with caution, because program group members in the pre-ZT group who were in Job Corps after March 1, 1995, became subject to the new rules. About 91 percent of program group enrollees in the pre-ZT group participated in Job Corps after March 1, 1995, and the pre-ZT group spent an average of 78 percent of their total time in Job Corps after the ZT policies took effect. Thus, impact estimates pertaining to the pre-ZT period are contaminated. Furthermore, program experiences could differ by season, and because of the limited sample intake period, the data are not available to compare impacts for those in pre-ZT and post-ZT groups who were recruited during the same time of year. Thus, while we find no effect of the new policies, the evidence is fairly weak.

## VII. WELFARE, CRIME, ILLEGAL DRUG USE, AND OTHER OUTCOMES

This chapter analyzes a range of other outcomes that Job Corps can influence. These analyses, in addition to those of education and training and employment and earnings, are designed to help assess the extent to which Job Corps achieves its goal of helping students become more responsible and productive.

The chapter addresses eight specific questions:

1. Does participation in Job Corps reduce dependence on welfare and other forms of public support?
2. Does Job Corps reduce involvement with the criminal justice system or the severity of crimes that program participants commit?
3. Does Job Corps reduce crimes committed against program participants?
4. Are participants less likely to use tobacco, alcohol, and illegal drugs?
5. Does Job Corps improve the overall health of participants?
6. Does Job Corps reduce the likelihood of bearing or fathering children while unmarried, or increase the likelihood of forming stable, long-term relationships?
7. Does Job Corps affect the use of child care and the types of arrangements that are used?
8. Does Job Corps influence the types of areas that participants move to after they leave the program?

To address these questions, we present program impacts on a diverse set of outcomes, both for the full sample and for key student subgroups.

As with education-related outcomes, and in contrast to employment-related outcomes, we expected program impacts on many of these nonlabor market outcomes to be largest during the early part of the follow-up period and perhaps to diminish later on. For example, we expected that program impacts on welfare receipt, crime, and illegal drug use would be substantial during the period when program group members were enrolled in Job Corps, and would diminish over time as the youths left the program.

Two factors led to these expectations. First, while participants are in Job Corps, their activities are restricted, their behavior is monitored, and their material needs are met. Consequently, there is less need for public assistance and less opportunity to engage in activities that lead to arrests. Second, we hypothesized that sample members would be less likely to receive public assistance, to engage in criminal activities, and to use illegal drugs as they matured and as their household incomes increased. With this maturation, we anticipated reductions in the size of program impacts over time.

Job Corps participation reduced the receipt of public assistance benefits. Overall, program group members reported receiving about $460 less in benefits (across several public assistance programs) than control group members, and this impact is statistically significant at the 1 percent level. Contrary to our expectations, however, impacts on public assistance receipt were not concentrated in the early part of the follow-up period but persisted throughout the period.

The estimated program impacts on the receipt of individual types of assistance were small and in many cases not statistically significant. The average number of months receiving Aid to Families with Dependent Children (AFDC) or Temporary Assistance for Needy Families (TANF) benefits differed by just 0.4 months (5.0 months for the program group and 5.4 for the control group). Control group members received food stamps for slightly more months on average than program group members (7.0 months, compared to 6.5 months). Impacts on the receipt of general assistance (GA), Supplemental Security Income (SSI), and WIC benefits and on the likelihood of being covered by public health insurance were small.

Job Corps participation significantly reduced arrest rates. About 33 percent of control group members were arrested during the 48-month follow-up period, compared to 29 percent of program group members (a statistically significant impact of about -4 percentage points per eligible applicant). The impact per participant was -5 percentage points, which translates to a 16 percent reduction in the arrest rate. Arrest rate reductions were largest during the first year after random assignment (when most program enrollees were in Job Corps). Interestingly, however, Job Corps also led to small arrest reductions during the later months of the follow-up period, after most youths had left the program.

Program group members were less likely to have arrest charges for nearly all categories of crimes. However, reductions were slightly larger for less serious crimes (such as disorderly conduct and trespassing).

Job Corps participation also reduced convictions and incarcerations resulting from a conviction. More than 25 percent of control group members were ever convicted during the follow-up period, compared to 22 percent of program group members. Similarly, Job Corps participation reduced the percentage incarcerated for convictions by 2 percentage points (from 18 percent to 16 percent).

Although the *level* of criminal activity differed substantially across youth subgroups, the *impacts* on crime outcomes were very similar (in particular, by gender and age). We find some differences, however, in crime impacts by residential status. Job Corps reduced arrest rates for male residents, female residents, and female nonresidents. However, the program had no effect for male nonresidents.

Job Corps participation led to reductions in crimes committed *against* program participants. As expected, the frequency of victimizations was reduced most during the in-program period, but the reductions persisted somewhat afterwards. Reductions were found for almost every crime type, and across most subgroups.

Job Corps had little effect on the self-reported use of tobacco, alcohol, and illegal drugs, for the full sample and for key subgroups. It also had little effect on time spent in drug treatment. Job Corps, however, significantly reduced the percentage of youths who rated their health as "poor" or "fair" at the time of the 12-, 30-, and 48-month interviews. At each interview, about 17.5 percent of the control group and 15.5 percent of the program group said their health was "poor" or "fair."

The program had no effect on fertility or custodial responsibility, either for the full sample or for key youth subgroups. About 38 percent of those in both the program and the control groups had a child during the follow-up period (49 percent of females and 31 percent of males). About two-thirds of all parents (and 42 percent of male parents) were living with all their children, and about 82 percent of males with children provided support for noncustodial children.

Job Corps participation, however, did have a small effect on promoting independent living at the 48-month interview point. A slightly smaller percentage of program group members were living with their parents (32 percent, compared to 35 percent of control group members), and a slightly larger percentage were living with a partner either married or unmarried (31 percent, compared to 29 percent). This same pattern holds for males and for females with and without children at baseline. Furthermore, the average distance between the zip codes of residence at application to Job Corps and at the 48-month interview was slightly larger for the program group (although the distance between the two zip codes was less than 10 miles for about three-quarters of both groups). The average characteristics of the counties of residence at 48 months, however, were similar for program and control group members.

Finally, Job Corps participation led to increases in the use of child care. Participants used an average of about 146 more hours of child care during the 48-month period than they would have if they had not enrolled in Job Corps. Impacts on child care use were positive during the first year after random assignment (when many program group members were enrolled in Job Corps) and during the fourth year (when employment impacts were the largest), but not in years 2 and 3. Impacts were found for females but not for males, because only a small percentage of males were living with their children and needed to find child care.

## A. RECEIPT OF PUBLIC ASSISTANCE AND OTHER SOURCES OF INCOME

Many sample members were dependent on public assistance before they applied to Job Corps. Nearly 60 percent of eligible applicants received some form of public income assistance in the year before random assignment (51 percent of males, 67 percent of females, and 88 percent of females with children) (Schochet 1998a). Thus, the extent to which Job Corps reduces participants' reliance on public assistance benefits, in both the short term and the longer term, is an important question.

Job Corps participants may experience a reduction in welfare receipt while they are enrolled in the program, because the program provides shelter (except to nonresidential students), food, and a small stipend. After they leave Job Corps, students may receive less public income support because of higher earnings. The program might also affect other sources of income, such as child support payments and income from friends.

In the following sections, we present impacts on the receipt of public assistance benefits and other sources of income for the full sample and for key youth subgroups.

### 1. Full Sample Results

The analysis relies on self-reports by sample members about assistance that they or their spouse or children who lived with them received from four groups of programs: (1) the federal AFDC program, which was replaced in 1996 with the TANF program; (2) the federal Food Stamp Program; (3) GA programs, which are locally funded efforts to provide income support to people who have no children and consequently do not qualify for AFDC/TANF; and (4) other federal programs that provide income support to people who are disabled, including the SSI and Social Security Retirement, Disability, or Survivor benefit (SSA) programs. In addition, respondents were asked to report on receipt of a variety of in-kind benefits (public health assistance, public housing, and WIC), as well as Unemployment Insurance (UI), child support, and support from family and friends.

In the first subsection below, we present data on total receipt of AFDC/TANF, food stamps, GA, and SSI/SSA benefits. The second subsection presents additional details by type of benefit received, including the in-kind programs and other sources of income.

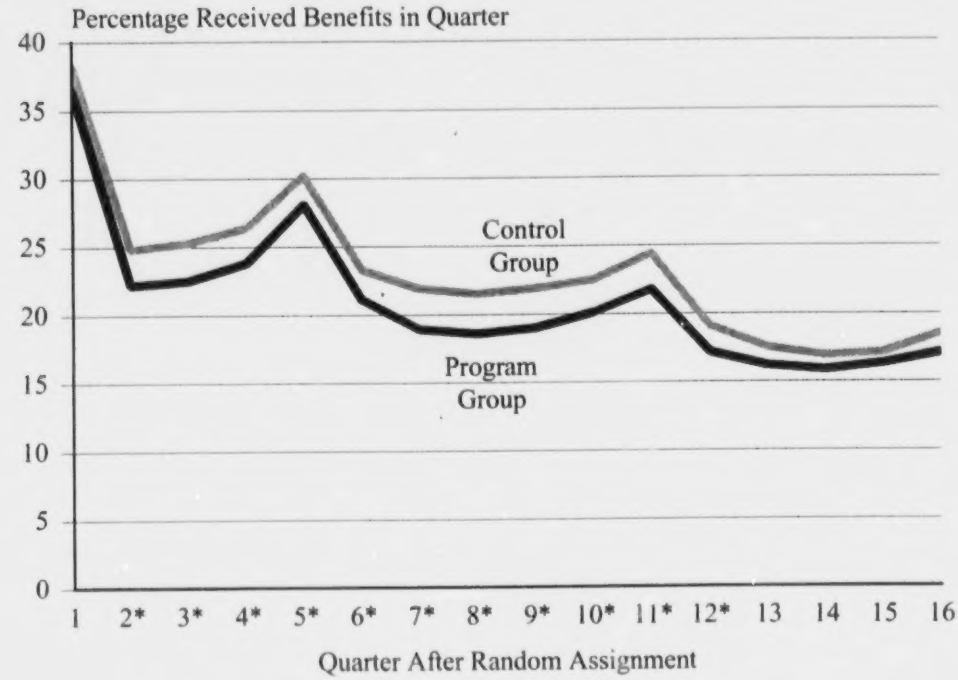
#### a. Impacts on Total Benefit Receipt

Figure VII.1 displays the percentage of program and control group members who received AFDC/TANF, food stamps, SSI/SSA, or GA during each quarter after random assignment. The differences between the program and control group percentages are estimated impacts per eligible applicant. The statistical significance of these impact estimates is indicated by asterisks along the horizontal axis. Table VII.1 displays more information on these impact estimates and presents impact findings on the number of months the youth received benefits and on the amount of benefits received. The estimates in the tables are displayed by quarter and by year after random assignment.

The *levels* of reported public assistance receipt were fairly constant from quarter to quarter, although there was a slight downward trend in average levels of receipt. For example, among the

FIGURE VII.1

RECEIPT OF AFDC/TANF, FOOD STAMP, SSI/SSA, OR GA BENEFITS, BY QUARTER



Source: Baseline and 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VII.1

## IMPACTS ON THE RECEIPT OF AFDC/TANF, FOOD STAMP, SSI/SSA, OR GA BENEFITS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Received Benefits, by Quarter After Random Assignment | | | | | | |
| 1 | 36.4 | 38.2 | -1.7* | 35.0 | -2.4* | -6.5 |
| 2 | 22.2 | 24.8 | -2.6*** | 19.9 | -3.6*** | -15.2 |
| 3 | 22.5 | 25.3 | -2.8*** | 20.2 | -3.9** | -16.1 |
| 4 | 23.8 | 26.4 | -2.5*** | 21.5 | -3.5*** | -14.1 |
| 5 | 28.1 | 30.2 | -2.0** | 26.2 | -2.8** | -9.8 |
| 6 | 21.1 | 23.3 | -2.2** | 19.2 | -3.0** | -13.6 |
| 7 | 18.9 | 21.9 | -3.0*** | 16.8 | -4.1*** | -19.6 |
| 8 | 18.6 | 21.5 | -2.9*** | 16.7 | -4.0*** | -19.3 |
| 9 | 19.0 | 21.9 | -2.9*** | 17.0 | -4.0*** | -19.0 |
| 10 | 20.1 | 22.5 | -2.4*** | 18.2 | -3.4*** | -15.8 |
| 11 | 21.8 | 24.4 | -2.6*** | 20.2 | -3.5*** | -14.9 |
| 12 | 17.2 | 19.1 | -1.9** | 15.7 | -2.6** | -14.4 |
| 13 | 16.2 | 17.5 | -1.3* | 15.1 | -1.8* | -10.8 |
| 14 | 15.9 | 16.9 | -0.9 | 14.9 | -1.3 | -8.1 |
| 15 | 16.3 | 17.1 | -0.8 | 15.6 | -1.2 | -7.0 |
| 16 | 17.1 | 18.5 | -1.4* | 16.1 | -2.0* | -10.8 |
| Percentage Received Benefits, by Period | | | | | | |
| All years | 54.5 | 57.5 | -3.0*** | 52.9 | -4.2*** | -7.4 |
| Year 1 | 40.2 | 42.8 | -2.5*** | 38.2 | -3.5*** | -8.5 |
| Year 2 | 33.1 | 36.0 | -3.0*** | 30.7 | -4.1*** | -11.8 |
| Year 3 | 26.0 | 29.0 | -3.0*** | 24.2 | -4.2*** | -14.7 |
| Year 4 | 21.7 | 22.8 | -1.0 | 20.6 | -1.4 | -6.5 |
| Month 48 | 15.8 | 17.5 | -1.7** | 14.8 | -2.4** | -13.9 |
| Average Number of Months Received Benefits, by Year | | | | | | |
| All years | 9.3 | 10.4 | -1.1*** | 8.5 | -1.5*** | -15.4 |
| 1 | 2.8 | 3.1 | -0.3*** | 2.5 | -0.5*** | -15.2 |
| 2 | 2.4 | 2.7 | -0.3*** | 2.2 | -0.4*** | -15.8 |
| 3 | 2.2 | 2.4 | -0.3*** | 2.0 | -0.4*** | -16.5 |
| 4 | 1.8 | 2.0 | -0.2* | 1.7 | -0.2* | -10.9 |
| Average Amount of Benefits Received, by Year (in Dollars) | | | | | | |
| All years | 3,696.0 | 4,155.7 | -459.8*** | 3,337.8 | -638.9*** | -16.1 |
| 1 | 1,109.8 | 1,225.9 | -116.2** | 1,002.6 | -161.4** | -13.9 |
| 2 | 978.7 | 1,101.6 | -122.9*** | 872.3 | -170.8*** | -16.4 |
| 3 | 893.3 | 1,001.4 | -108.1*** | 798.0 | -150.2*** | -15.8 |
| 4 | 745.5 | 825.6 | -80.1** | 694.3 | -111.3** | -13.8 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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TABLE VII.1 *(continued)*

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[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

control group, the average percentage receiving public assistance in each quarter was 29 percent during the first year after random assignment, 24 percent in year 2, 22 percent in year 3, and 18 percent in year 4.[1]

The *impacts* on reported public assistance receipt were constant from quarter to quarter throughout the first three years of the follow-up period but were somewhat smaller during year 4. The rates of receipt were 2 to 3 percentage points lower among the program group than among the control group in each quarter in years 1 to 3, and the differences are statistically significant. In percentage terms, the impacts were about 15 to 20 percent per participant. In year 4, the quarterly impacts on the rates of receipt were about half as large.

As one would expect from this pattern, total months of receipt during the 48-month follow-up period was about 1.1 months lower on average for the program group (9.3 months, compared to 10.4 months for the control group), and average benefits were about $460 lower (about $3,700 for the program group and $4,160 for the control group). As described next, this $460 impact on total benefits was due to the sum of small impacts on the amount of AFDC/TANF, food stamp, SSI/SSA, and GA benefits received.

### b. Impacts by Type of Benefit Receipt

Job Corps participation had a small effect on the receipt of benefits from programs that provided income support to families with children (AFDC/TANF) during the follow-up period (Figure VII.2

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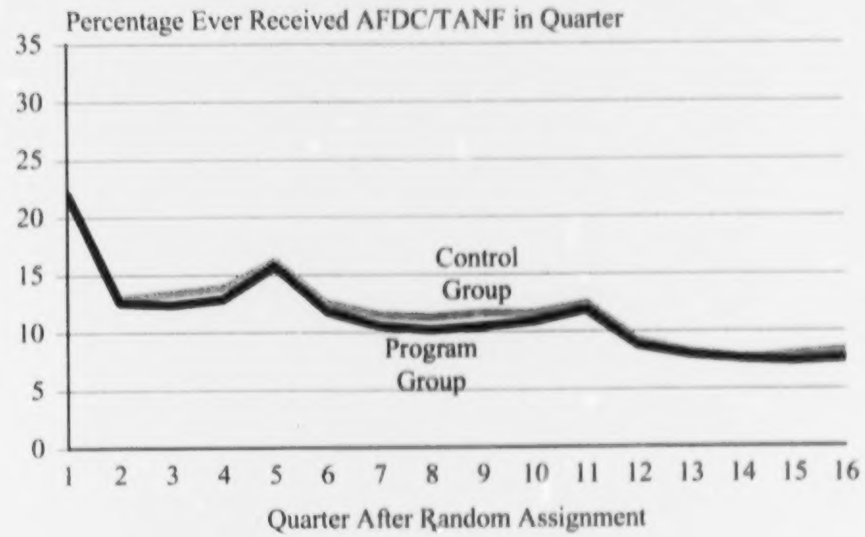
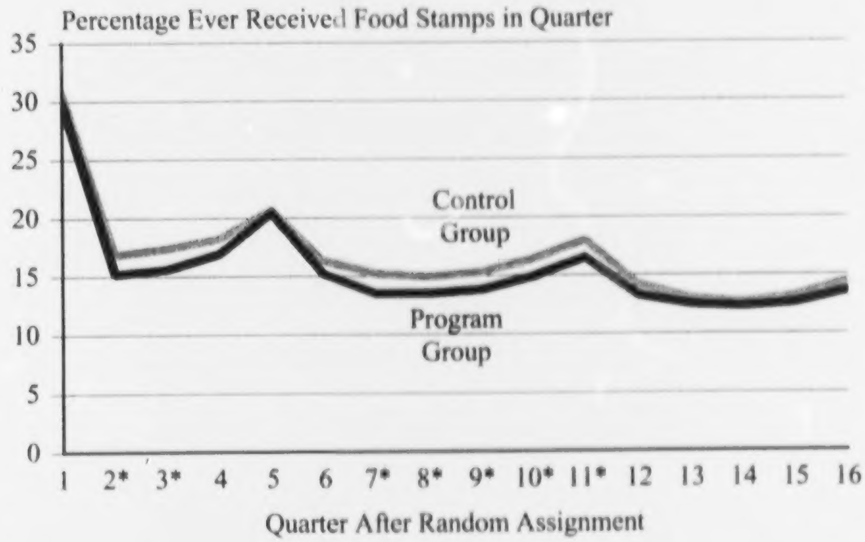
[1] The spikes in the benefit receipt rate in quarters 1, 5, and 11 are likely due to a "seam problem" in the interviews. Quarter 1 is the last quarter covered by the baseline interview and the first quarter covered by the 12-month interview. Similarly, quarter 5 is the last quarter covered by the 12-month interview and the first quarter covered by the 30-month interview. Finally, quarter 11 is the last quarter covered by the 30-month interview and the first quarter covered by the 48-month interview. Some respondents who reported at an interview that they had recently received benefits may have forgotten during the next interview that they had been receiving these benefits.

FIGURE VII.2

## RECEIPT OF AFDC/TANF AND FOOD STAMP BENEFITS, BY QUARTER





Source: Baseline and 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

and Table VII.2). About 33 percent of each research group reported ever receiving AFDC/TANF benefits during the follow-up period. The control group was slightly more likely to have received benefits in each quarter after quarter 1, although the estimated impacts are not statistically significant at the 5 percent level. The control group received an average of $123 more AFDC/TANF benefits than the program group over the 48-month period ($1,608, compared to $1,485).

Job Corps participation had a modest effect on the receipt of food stamp benefits (Figure VII.2 and Table VII.3). More than 48 percent of control group members ever received food stamps during the 48 months, compared to less than 46 percent of program group members (a statistically significant impact of about 3 percentage points per eligible applicant). Job Corps participants received benefits for about two weeks (0.7 months) less on average than they would have if they had not enrolled in the program (an 11 percent reduction), and received an average of about $100 less in benefits (an 8 percent reduction). The food stamp benefit receipt rates declined only slightly over time, and the impacts were similar during year 1, when many program group members were enrolled in the program, and during years 2 and 3, when many had left the program. The impacts persisted into year 4, although they were smaller.

Receipt of GA benefits was rare (Table VII.4). During the 48-month follow-up period, about 4 percent of each group received GA benefits, although slightly fewer program group members did so (3.5 percent of the program group and 4.3 percent of the control group). Impacts were small on the amount of GA benefits received.

Receipt of SSI/SSA benefits was more common than receipt of GA benefits, and impacts on the SSI/SSA measures were larger. For example, 10.9 percent of the control group and 9.3 percent of the program group reported receiving SSI/SSA benefits, a statistically significant reduction of 1.6 percentage points per eligible applicant (2.3 percentage points per participant). Reductions in the

## TABLE VII.2

## IMPACTS ON THE RECEIPT OF AFDC/TANF BENEFITS

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Percentage Received AFDC/ TANF Benefits, by Quarter After Random Assignment** | | | | | | |
| 1 | 22.1 | 22.0 | 0.1 | 21.3 | 0.2 | 0.9 |
| 2 | 12.6 | 13.0 | -0.5 | 11.4 | -0.6 | -5.2 |
| 3 | 12.4 | 13.4 | -1.0 | 11.2 | -1.3 | -10.5 |
| 4 | 12.9 | 13.9 | -1.1 | 11.6 | -1.5 | -11.3 |
| 5 | 15.7 | 16.2 | -0.6 | 14.5 | -0.8 | -5.2 |
| 6 | 11.8 | 12.5 | -0.7 | 10.8 | -1.0 | -8.6 |
| 7 | 10.6 | 11.5 | -1.0 | 9.4 | -1.3 | -12.3 |
| 8 | 10.2 | 11.3 | -1.1* | 9.1 | -1.6* | -14.8 |
| 9 | 10.5 | 11.6 | -1.1* | 9.6 | -1.5* | -13.2 |
| 10 | 10.9 | 11.6 | -0.7 | 10.1 | -1.0 | -9.0 |
| 11 | 11.9 | 12.4 | -0.5 | 11.1 | -0.7 | -6.0 |
| 12 | 8.8 | 9.2 | -0.4 | 8.0 | -0.6 | -6.9 |
| 13 | 8.0 | 8.2 | -0.2 | 7.6 | -0.2 | -3.1 |
| 14 | 7.6 | 7.6 | 0.0 | 7.2 | -0.1 | -0.7 |
| 15 | 7.4 | 8.0 | -0.6 | 7.4 | -0.8 | -9.6 |
| 16 | 7.6 | 8.3 | -0.7 | 7.7 | -0.9 | -10.5 |
| **Percentage Received AFDC/ TANF Benefits, by Period** | | | | | | |
| All years | 33.2 | 33.5 | -0.3 | 32.0 | -0.4 | -1.3 |
| Year 1 | 23.9 | 24.4 | -0.5 | 22.6 | -0.8 | -3.2 |
| Year 2 | 18.2 | 19.6 | -1.4* | 16.7 | -2.0* | -10.7 |
| Year 3 | 14.4 | 15.2 | -0.9 | 13.4 | -1.2 | -8.2 |
| Year 4 | 10.5 | 10.9 | -0.4 | 10.2 | -0.6 | -5.5 |
| Month 48 | 7.1 | 7.8 | -0.7 | 7.1 | -1.0 | -12.3 |
| **Average Number of Months Received AFDC/TANF Benefits, by Year** | | | | | | |
| All years | 5.0 | 5.4 | -0.4* | 4.6 | -0.5* | -10.2 |
| 1 | 1.6 | 1.7 | -0.1 | 1.4 | -0.1 | -8.7 |
| 2 | 1.3 | 1.4 | -0.1 | 1.2 | -0.1 | -10.0 |
| 3 | 1.1 | 1.2 | -0.1 | 1.1 | -0.1 | -10.3 |
| 4 | 0.9 | 0.9 | 0.0 | 0.8 | -0.1 | -7.2 |
| **Average Amount of AFDC/ TANF Benefits Received, by Year (in Dollars)** | | | | | | |
| All years | 1,484.7 | 1,607.7 | -123.0* | 1,366.9 | -170.9* | -11.1 |
| 1 | 458.7 | 483.0 | -24.3 | 415.3 | -33.8 | -7.5 |
| 2 | 388.4 | 418.6 | -30.1 | 350.9 | -41.9 | -10.7 |
| 3 | 348.6 | 375.1 | -26.5 | 322.8 | -36.9 | -10.3 |
| 4 | 266.1 | 282.7 | -16.6 | 261.8 | -23.1 | -8.1 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48 follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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TABLE VII.2 *(continued)*

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[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE VII.3

## IMPACTS ON THE RECEIPT OF FOOD STAMP BENEFITS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Received Food Stamp Benefits, by Quarter After Random Assignment | | | | | | |
| 1 | 29.8 | 30.7 | -0.9 | 28.2 | -1.3 | -4.3 |
| 2 | 15.2 | 16.8 | -1.6** | 12.9 | -2.3** | -14.9 |
| 3 | 15.6 | 17.4 | -1.7** | 13.4 | -2.4** | -15.1 |
| 4 | 16.9 | 18.2 | -1.3* | 14.7 | -1.8* | -11.1 |
| 5 | 20.4 | 20.7 | -0.3 | 18.4 | -0.4 | -2.3 |
| 6 | 15.2 | 16.3 | -1.1 | 13.6 | -1.5 | -10.1 |
| 7 | 13.5 | 15.2 | -1.8*** | 11.7 | -2.5*** | -17.4 |
| 8 | 13.5 | 14.9 | -1.4** | 12.0 | -2.0** | -14.1 |
| 9 | 13.8 | 15.3 | -1.5** | 12.2 | -2.0** | -14.3 |
| 10 | 14.9 | 16.4 | -1.6** | 13.3 | -2.2** | -14.2 |
| 11 | 16.5 | 18.0 | -1.5** | 15.2 | -2.0** | -11.8 |
| 12 | 13.3 | 14.2 | -1.0 | 12.0 | -1.3 | -10.0 |
| 13 | 12.5 | 13.0 | -0.5 | 11.4 | -0.7 | -5.8 |
| 14 | 12.3 | 12.7 | -0.4 | 11.5 | -0.5 | -4.5 |
| 15 | 12.6 | 13.2 | -0.6 | 12.0 | -0.8 | -6.4 |
| 16 | 13.6 | 14.4 | -0.8 | 12.7 | -1.1 | -7.8 |
| Percentage Received Food Stamps, by Year | | | | | | |
| All years | 45.7 | 48.3 | -2.7*** | 44.0 | -3.7*** | -7.7 |
| Year 1 | 33.0 | 34.5 | -1.5 | 30.9 | -2.1 | -6.3 |
| Year 2 | 24.6 | 25.9 | -1.3 | 22.4 | -1.9 | -7.7 |
| Year 3 | 20.3 | 22.2 | -1.9** | 18.8 | -2.7** | -12.4 |
| Year 4 | 17.2 | 17.7 | -0.5 | 16.2 | -0.6 | -3.8 |
| Month 48 | 12.4 | 13.4 | -0.9 | 11.6 | -1.3 | -10.2 |
| Average Number of Months Received Food Stamps, by Year | | | | | | |
| All years | 6.5 | 7.0 | -0.5** | 5.7 | -0.7** | -10.9 |
| 1 | 2.0 | 2.1 | -0.2** | 1.7 | -0.2** | -12.3 |
| 2 | 1.7 | 1.8 | -0.1* | 1.5 | -0.2* | -10.4 |
| 3 | 1.6 | 1.7 | -0.1* | 1.4 | -0.2* | -11.8 |
| 4 | 1.4 | 1.5 | -0.1 | 1.3 | -0.1 | -6.6 |
| Average Amount of Food Stamps Received, by Year (in Dollars) | | | | | | |
| All years | 1,326.0 | 1,399.6 | -73.6 | 1,151.6 | -102.3 | -8.2 |
| 1 | 390.5 | 414.3 | -23.8 | 339.6 | -33.0 | -8.9 |
| 2 | 338.1 | 358.2 | -20.1 | 293.6 | -28.0 | -8.7 |
| 3 | 328.5 | 346.3 | -17.8 | 288.1 | -24.7 | -7.9 |
| 4 | 306.0 | 315.8 | -9.8 | 277.3 | -13.6 | -4.7 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

TABLE VII.3 *(continued)*

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[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE VII.4

## IMPACTS ON THE RECEIPT OF GA AND SSI/SSA BENEFITS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Received GA Benefits, by Year | | | | | | |
| All years | 3.5 | 4.3 | -0.8** | 3.1 | -1.1** | -26.5 |
| 1 | 1.5 | 1.7 | -0.2 | 1.4 | -0.3 | -17.1 |
| 2 | 1.9 | 2.1 | -0.2 | 1.7 | -0.3 | -17.0 |
| 3 | 1.3 | 1.6 | -0.3 | 1.1 | -0.4 | -24.9 |
| 4 | 1.1 | 1.2 | -0.2 | 0.9 | -0.3 | -21.6 |
| Average Number of Months Ever Received GA | 0.4 | 0.4 | 0.0 | 0.3 | -0.1 | -17.2 |
| Average Amount of GA Benefits Ever Received (in Dollars) | 82.3 | 108.3 | -26.0* | 74.7 | -36.1* | -32.6 |
| Percentage Received SSI/SSA Benefits, by Year | | | | | | |
| All years | 9.3 | 10.9 | -1.6*** | 8.8 | -2.3*** | -20.6 |
| 1 | 5.3 | 6.5 | -1.2*** | 5.0 | -1.7*** | -25.0 |
| 2 | 6.7 | 8.3 | -1.6*** | 6.3 | -2.2*** | -25.5 |
| 3 | 4.6 | 5.9 | -1.3*** | 4.1 | -1.8*** | -30.5 |
| 4 | 3.5 | 4.2 | -0.8** | 3.2 | -1.1*** | -25.0 |
| Average Number of Months Ever Received SSI/SSA Benefits | 1.8 | 2.3 | -0.5*** | 1.6 | -0.7*** | -30.3 |
| Average Amount of SSI/SSA Benefits Ever Received (in Dollars) | 767.8 | 994.2 | -226.4*** | 689.4 | -314.6*** | -31.3 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

number of months of receipt (0.5 months) and total benefits received ($226) translate to 31 percent reductions due to program participation.

We find few differences in the receipt of other in-kind assistance (Table VII.5). About 35 percent of program and control group members were covered by a public health insurance program (and about one-third percent by Medicaid) at each interview point.[2,3] About half the females in each group received WIC benefits. About 15 percent of sample members lived in public housing at each interview point.

Control group members were slightly more likely than program group members to receive UI benefits, although only about 6 percent of both groups received them (Table E.1). Control group members received an average of about $36 more in UI benefits than program group members, and this impact is statistically significant. The negative impacts on the receipt of UI benefits occurred early in the follow-up period, when control group members were employed more than program group members.

Finally, the receipt of other types of income was not affected by Job Corps participation (Table E.1). Impacts on income from child support payments, friends, and other sources were small and not statistically significant.

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[2] Those receiving AFDC/TANF were eligible for Medicaid. Thus, we assumed that those receiving AFDC/TANF benefits at the interview dates were covered by Medicaid even if they reported that they were not covered. The impact results are very similar if we do not make this assumption (in which case about 29 percent rather than 33 percent of both groups were covered by Medicaid).

[3] Among those covered by health insurance at 12 months, a slightly lower proportion of program than control group members reported being covered by Medicaid, and a slightly higher proportion by another public assistance program. We observe this pattern possibly because some program group enrollees may have reported that they were covered by health insurance through Job Corps. We do not observe this pattern at 30 or 48 months, because nearly all program group participants had left Job Corps by then.

## TABLE VII.5

## IMPACTS ON PUBLIC HEALTH INSURANCE COVERAGE AND THE RECEIPT OF WIC AND PUBLIC HOUSING BENEFITS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Type of Public Health Insurance Coverage at the 12-Month Interview | | | | | | |
| Not covered | 64.8 | 63.7 | 1.1 | 66.5 | 1.5 | 2.4 |
| Medicaid | 30.2 | 31.8 | -1.6 | 28.4 | -2.3 | -7.4 |
| Another public health assistance program | 5.0 | 4.5 | 0.5 | 5.1 | 0.7 | 16.2 |
| Type of Public Health Insurance Coverage at the 30-Month Interview | | | | | | |
| Not covered | 65.2 | 64.3 | 0.8 | 66.5 | 1.2 | 1.8 |
| Medicaid | 32.8 | 33.0 | -0.2 | 31.3 | -0.3 | -0.9 |
| Another public health assistance program | 2.0 | 2.7 | -0.6 | 2.2 | -0.9 | -28.9 |
| Type of Public Health Insurance Coverage at the 48-Month Interview | | | | | | |
| Not covered | 66.0 | 64.9 | 1.1 | 67.1 | 1.5 | 2.3 |
| Medicaid | 31.2 | 32.3 | -1.1 | 30.1 | -1.5 | -4.6 |
| Another public health assistance program | 2.7 | 2.8 | 0.0 | 2.8 | -0.1 | -2.0 |
| Percentage Received WIC Benefits (for Females Only), by Year | | | | | | |
| All years | 52.7 | 51.0 | 1.7 | 52.4 | 2.4 | 4.8 |
| 1 | 18.6 | 19.7 | -1.1 | 17.0 | -1.5 | -7.9 |
| 2 | 33.4 | 34.3 | -0.9 | 32.0 | -1.2 | -3.7 |
| 3 | 37.9 | 37.4 | 0.5 | 38.3 | 0.8 | 2.0 |
| 4 | 35.3 | 31.6 | 3.7** | 35.8 | 5.1** | 16.6 |
| Average Number of Months Ever Received WIC Benefits (for Females Only) | 11.3 | 11.1 | 0.2 | 11.0 | 0.3 | 2.9 |
| Percentage Lived in Public Housing | | | | | | |
| At 12 months | 15.0 | 16.1 | -1.0 | 14.3 | -1.4 | -9.1 |
| At 30 months | 15.3 | 16.1 | -0.8 | 15.0 | -1.2 | -7.1 |
| At 48 months | 13.7 | 14.2 | -0.5 | 13.6 | -0.7 | -4.7 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

TABLE VII.5 *(continued)*

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[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## 2. Subgroup Results

In our sample, young men, young women with no children at baseline, and young women with children at baseline were likely to have had very different experiences with public assistance programs. The young men were much less likely than the females to have had children at random assignment (11 percent, compared to 29 percent) or to have lived with their children, and, as discussed later in this chapter, they were much less likely to have had children during the follow-up period (31 percent, compared to 49 percent). Thus, we expected the male youths to be less reliant than the female youths on welfare in general and on AFDC/TANF benefits in particular. To be sure, some males may have reported receiving AFDC/TANF benefits if they lived with parents and younger siblings or if they formed their own households that contained children. However, we expected that food stamps, GA, or SSI/SSA benefits would constitute a large share of welfare receipt among male recipients, because males could have been eligible for these benefits whether or not they lived with children. On the other hand, more than 45 percent of young women with no children at baseline gave birth during the 48-month period and thus could have become eligible for AFDC/TANF (and WIC) benefits when their children were born (or shortly before). Thus, we might expect that these females would be more reliant on AFDC/TANF benefits. Finally, the young women who had children at the time they applied for Job Corps may have received AFDC/TANF while in Job Corps if they were nonresidential students, or their children may have received it while they were attending Job Corps if they were residential students. Thus, this group was expected to be particularly dependent on public assistance. Although the preceding section provided an overview of program impacts on receipt of public assistance, it unavoidably obscures differences in the experiences of these groups with divergent needs and circumstances.

This section presents impacts on public assistance receipt for males and females with and without children at random assignment. Figure VII.3 displays the percentage of program group and control group members in each of these subgroups who ever received key types of public assistance during each quarter of the follow-up period. Figure VII.4 summarizes data on the composition of benefits received for each subgroup, and Tables E.2 to E.4 display more details on the impact findings. The section concludes with a brief discussion of impacts on key welfare outcomes for other youth subgroups.

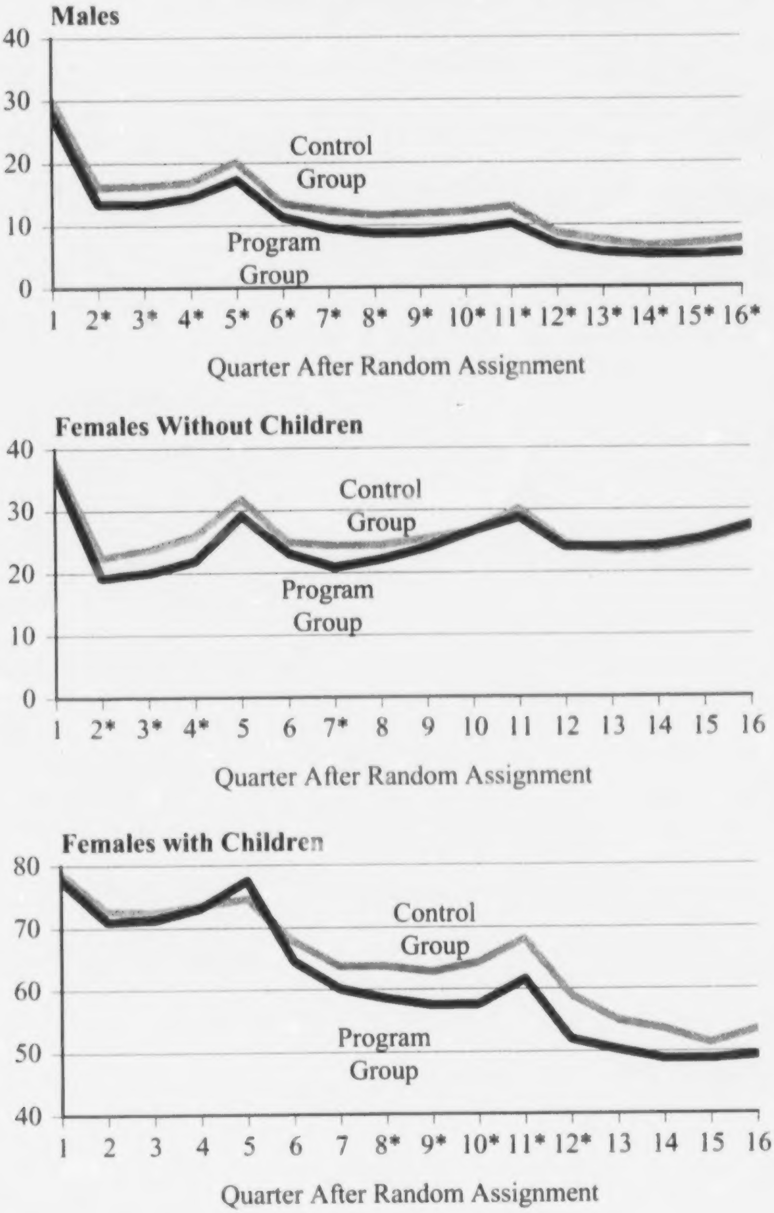
### a. Impacts for Males

The level of public assistance receipt among male control group members declined somewhat during the 48-month follow-up period. During the first year, about 20 percent of control group males received public assistance per quarter. The figure was about 14 percent during the second year, about 11 percent in year 3, and 7 percent in year 4. Approximately 53 percent of the total amount of benefits that the male control group members received was from AFDC/TANF and food stamps, while about 43 percent was from SSI/SSA, and the balance was from GA.

Impacts on public assistance receipt for males were nearly constant throughout the follow-up period. The difference in the percentage receiving assistance was about 2 to 3 percentage points per quarter. Similarly, the impact on benefits per month was about $9 per month during the first three years of the follow-up period, and was about $7.5 during year 4. It appears likely that some males in the program group stopped receiving public assistance when they enrolled in Job Corps (because nearly all enrolled as residential students) and continued not receiving it after they left the program.

FIGURE VII.3

## PERCENTAGE WHO RECEIVED AFDC/TANF, FOOD STAMP, SSI/SSA, OR GA BENEFITS, FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN, BY QUARTER

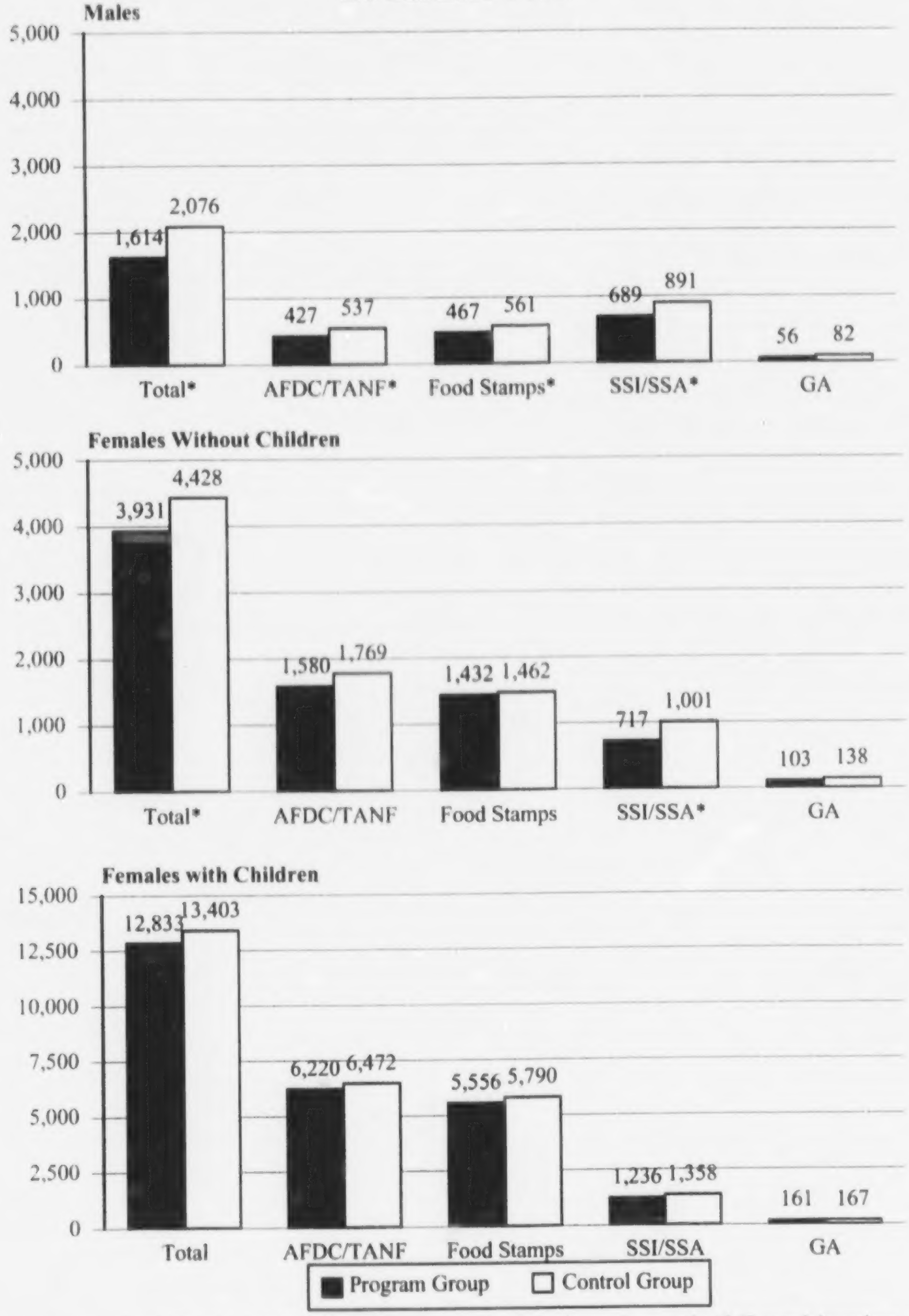


Source: Baseline and 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## FIGURE VII.4

## AVERAGE DOLLAR VALUE OF PUBLIC ASSISTANCE BENEFITS RECEIVED BY MALES AND BY FEMALES WITH AND WITHOUT CHILDREN, BY BENEFIT TYPE



Source: Baseline and 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

Note: The total benefit figures do not equal the sum of the benefit figures by type because of missing values.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

### b. Impacts for Females Without Children

In the control group, welfare receipt among female applicants who had no children was essentially unchanged over the follow-up period. Despite quarter-to-quarter fluctuations, an average of 26 percent of the control group received public assistance in each quarter during the follow-up period. Nearly 75 percent of the total value of benefits these control group members reported receiving was from AFDC/TANF or food stamps.

In contrast to the time profile of impacts on public assistance receipt among the males, impacts among females without children were larger early but declined over time. The impacts on receipt in each quarter were about 3.5 percentage points during the first 12 months and declined to 2.5 percentage points during the second 12 months. During the last two years of the follow-up period, they were small and not statistically significant. Similarly, the impact on benefits per month declined from $16 in year 1 to $13 in year 2 to $4 in years 3 and 4. It appears that public assistance receipt was lower for the program group in the first year because the women were in Job Corps. After the first year, however, the rates of receipt among the program group increased as the women had children (as nearly one-half did during the 48-month follow-up period), while the rates of welfare receipt among the control group remained unchanged.

### c. Impacts for Females with Children

Females with children at baseline exhibited patterns of public assistance receipt and impacts on these outcomes that differed from those of males and females without children. These differences stem in large measure from the fact that a large fraction of females with children are *nonresidential* students. Not surprisingly, public assistance receipt was much more common for females with children than for males and females without children. About three-quarters of control group females with children typically received public assistance during each quarter in the first year after random assignment. The benefit receipt rate declined to just under two-thirds by the end of year 2 and to just over one-half by the end of year 4, but it remained high. As one would expect, about 90 percent of the public assistance that females with children received over the 48-month follow-up period was AFDC/TANF or food stamps.

The time profile of impacts on the public assistance of females with children also differs from the profiles for males and females without children. In contrast to males (for whom impacts were constant over time) and to females with no children (for whom impacts declined), the impacts on the public assistance receipt of females with children were larger during the postprogram period than during the in-program period. During the first year, the average difference in the percentage receiving public assistance in each quarter was about 1 percentage point. This average difference increased to about 3 percentage points during the second year and to 6.5 percentage points during the third year. In year 4, the average difference was about 4 percentage points per quarter.

It appears that program group members relied on public assistance to support them and their children while they attended Job Corps, but that some were able to leave public assistance during the postprogram period as their earnings increased.

### d. Impacts for Other Subgroups

There were few differences in impacts on public assistance measures for most other key subgroups defined by youth characteristics (Table E.5). Impact estimates were similar by age, high school credential status, arrest experience, and whether the youth applied before or after the ZT policies took effect. There is some evidence, however, that impacts were slightly larger for whites and African Americans than for other racial and ethnic subgroups, which is consistent with our finding that impacts on employment and earnings were larger for whites and African Americans.

## B. INVOLVEMENT WITH THE CRIMINAL JUSTICE SYSTEM

Job Corps serves many youths who have been involved with the criminal justice system. Nearly 27 percent of eligible program applicants in our research sample reported that they had been arrested or charged with a delinquency or criminal complaint before random assignment (Schochet 1998a). The arrest rate was even higher (about one-third) for males and those 16 and 17 years old at application to the program. More than 5 percent reported having been arrested for serious crimes (including murder, assault, robbery, or burglary), and the figure is nearly 8 percent for males. About 17 percent were convicted, and about 8 percent (and 10.4 percent of males) ever served time in jail. Because of the high costs of crime both to victims (due to injury and lost property) and to taxpayers in the form of criminal justice system costs, potential reductions in criminal activities from participation in Job Corps could be an important component of program benefits.

Job Corps is expected to reduce the incidence and severity of crimes committed while students are enrolled in the program, because participants' activities are restricted, their behavior is monitored, and their material needs are met. Because Job Corps students spend most of their time at their center and many centers are in isolated areas, students' opportunities to get in trouble with the law are limited. In addition, intensive instructional and recreational activities during the day leave little time for anything else. After students leave the program, reductions in crime are expected to continue because of skills learned in the program, but reductions may be lower than during the in-program period, because the highly structured day and close monitoring will have been removed.

This section presents impacts on self-reported arrests, convictions, and incarcerations resulting from convictions for crimes committed during the 48 months after random assignment. It presents data for the full sample and for key youth subgroups. The analysis was conducted using self-reported data on arrest dates, arrest charges, the disposition of arrest charges, and jail time for convictions.[4]

A separate report (Needels et al. 2000) uses official crime records from North Carolina and Texas to present impact results on arrests and convictions covering the 30-month period after random assignment. In general, the 30-month impact findings based on the official records are similar to those obtained using survey data for those who lived in North Carolina and Texas. Each data source has both strengths and weaknesses, and it is unclear which data source is more accurate for estimating impacts. However, the similarity of the findings using the two data sources suggests that reliance on self-reports for the impact analysis is unlikely to have created serious bias in the survey-based estimates of crime impacts.

Job Corps participation led to about a 16 percent reduction in the arrest rate, the conviction rate, and the incarceration rate for convictions during the 48-month period after random assignment. In addition, the reductions were spread fairly uniformly across different types of crimes. Job Corps reduced criminal activities for most groups of students, although no crime impacts were found for male nonresidents.

### 1. Impacts on Arrest Rates

Figure VII.5 displays the percentage of program and control group members who were arrested or charged with a delinquency or criminal complaint, by quarter after random assignment. The

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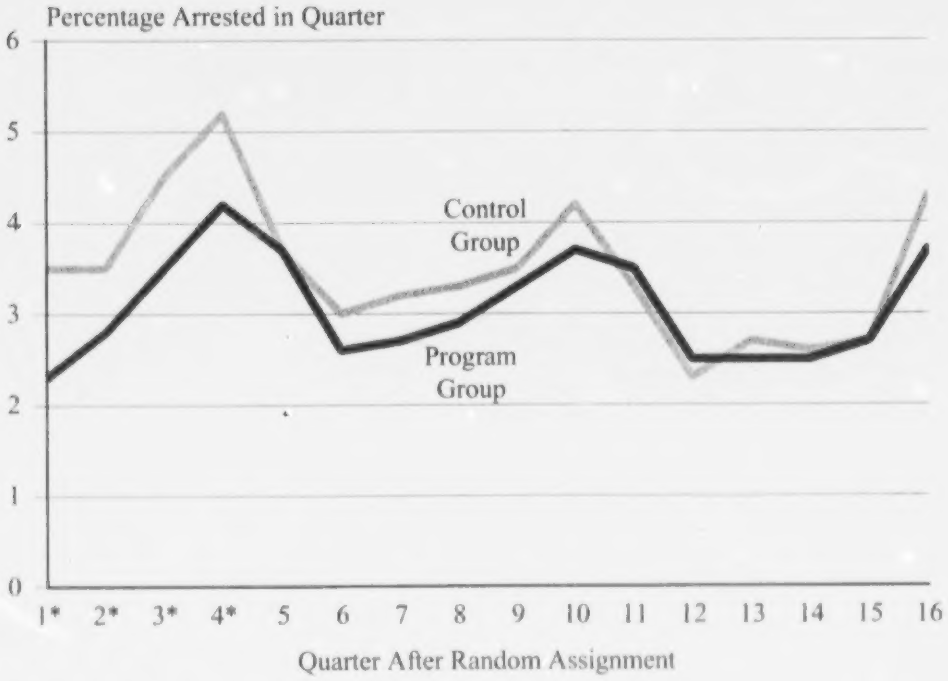
[4] The analysis used crime data from the 12-, 30-, and 48-month follow-up interviews. The baseline interview data also contain crime information covering the follow-up period (that is, the period between the random assignment and the baseline interview dates). However, the baseline data do not contain complete conviction and incarceration information, and thus we did not use them in the analysis. The 12-month interview (or the 30-month or 48-month interview for those who did not complete a 12-month interview) collected complete crime information from the random assignment date onwards (and not from the baseline interview date). Thus, we have complete self-reported crime information covering the 48-month follow-up period.

FIGURE VII.5

ARREST RATES, BY QUARTER



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

differences between the arrest rates by research status are estimated impacts per eligible applicant. Table VII.6 provides detailed information on these estimates and on impact estimates for other arrest-related outcomes.

Unexpectedly, the arrest rate for the control group declined only slightly over time as sample members matured. The average control group arrest rate per quarter was 4.2 percent during the first year after random assignment, and it declined to 3.3 percent in years 2 and 3 and 3.1 percent in year 4.[5]

Overall, about 33 percent of control group members were arrested at some point during the follow-up period (Table VII.6). About 18 percent of control group members (and 55 percent of those arrested) were arrested more than once, and nearly one-half of those arrested were arrested within the first year after random assignment.

Job Corps participation led to statistically significant reductions in the arrest rate. While 32.6 percent of control group members were arrested during the 48-month follow-up period, 28.8 percent of program group members were arrested in the same period (a statistically significant impact of -3.7 percentage points per eligible applicant). The arrest rate for program participants was 27.6 percent, and we estimate this to be 5.2 percentage points lower than it would have been if the participants had not enrolled in the program. This impact corresponds to a 16 percent reduction in the arrest rate due to program participation.

Reductions in the arrest rate were largest during the first year after random assignment (when most program enrollees were in Job Corps). However, Job Corps participation also led to reductions in the arrest rate after the youths left the program. For example, arrests were reduced by more than

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[5] The arrest rates spiked in quarters 4, 10, and 16 because youths were probably better able to recall recent arrests than less recent arrests during the 12-, 30-, and 48-month follow-up interviews.

## TABLE VII.6

## IMPACTS ON ARRESTS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1 | 2.3 | 3.5 | -1.3*** | 1.6 | -1.7*** | -52.4 |
| 2 | 2.8 | 3.5 | -0.7** | 2.4 | -1.0** | -29.8 |
| 3 | 3.5 | 4.5 | -1.1*** | 3.2 | -1.5*** | -32.4 |
| 4 | 4.2 | 5.2 | -1.0** | 3.7 | -1.4** | -27.5 |
| 5 | 3.7 | 3.7 | 0.0 | 3.3 | 0.0 | 1.1 |
| 6 | 2.6 | 3.0 | -0.4 | 2.5 | -0.5 | -17.2 |
| 7 | 2.7 | 3.2 | -0.5* | 2.7 | -0.8* | -21.8 |
| 8 | 2.9 | 3.3 | -0.4 | 3.0 | -0.5 | -15.2 |
| 9 | 3.3 | 3.5 | -0.2 | 3.5 | -0.3 | -9.1 |
| 10 | 3.7 | 4.2 | -0.5 | 3.7 | -0.7 | -15.3 |
| 11 | 3.5 | 3.3 | 0.2 | 3.0 | 0.2 | 7.7 |
| 12 | 2.5 | 2.3 | 0.2 | 2.4 | 0.2 | 10.1 |
| 13 | 2.5 | 2.7 | -0.2 | 2.6 | -0.2 | -8.6 |
| 14 | 2.5 | 2.6 | -0.1 | 2.4 | -0.2 | -7.5 |
| 15 | 2.7 | 2.7 | -0.1 | 2.6 | -0.1 | -3.1 |
| 16 | 3.7 | 4.3 | -0.6 | 3.8 | -0.8 | -17.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years | 28.8 | 32.6 | -3.7*** | 27.6 | -5.2*** | -15.8 |
| Year 1 | 11.1 | 14.1 | -3.1*** | 9.6 | -4.3*** | -30.8 |
| Year 2 | 10.5 | 11.3 | -0.8 | 10.0 | -1.2 | -10.5 |
| Year 3 | 11.1 | 11.4 | -0.4 | 10.7 | -0.5 | -4.7 |
| Year 4 | 9.6 | 10.3 | -0.7 | 9.7 | -0.9 | -8.8 |
| Number of Times Arrested (Percentages) | | | | | | |
| 0 | 71.8 | 67.8 | 4.0***[d] | 73.0 | 5.5***[d] | 8.2 |
| 1 | 12.7 | 14.2 | -1.5 | 12.8 | -2.1 | -13.9 |
| 2 | 6.5 | 8.1 | -1.6 | 5.9 | -2.2 | -26.9 |
| 3 or more | 9.0 | 9.9 | -0.9 | 8.3 | -1.3 | 28.8 |
| Average Number of Arrests, by Year | | | | | | |
| All years | 0.66 | 0.75 | -0.09*** | 0.62 | -0.12*** | 11.1 |
| Year 1 | 0.17 | 0.23 | -0.06*** | 0.14 | -0.08*** | 9.6 |
| Year 2 | 0.16 | 0.17 | -0.01 | 0.16 | -0.02 | -10.5 |
| Year 3 | 0.18 | 0.18 | 0.00 | 0.17 | -0.01 | -3.3 |
| Year 4 | 0.15 | 0.17 | -0.02 | 0.16 | -0.02 | -12.4 |
| Months Until First Arrested (Percentages) | | | | | | |
| Not arrested | 71.1 | 67.0 | 4.0***[d] | 72.3 | 5.6***[d] | 8.4 |
| Less than 12 | 11.2 | 14.4 | -3.2 | 9.8 | -4.4 | -31.2 |
| 12 to 24 | 7.2 | 8.2 | -1.0 | 7.2 | -1.4 | -16.3 |
| 25 to 36 | 6.3 | 5.9 | 0.4 | 6.3 | 0.5 | 8.8 |
| 36 to 48 | 4.2 | 4.4 | -0.2 | 4.4 | -0.3 | -5.5 |

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TABLE VII.6 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Months Until First Arrested for Those Arrested | 16.4 | 15.0 | 1.4*** | 17.1 | 2.0*** | 12.9 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

30 percent during year 1, and this impact is statistically significant at the 1 percent level. However, the arrest rates in years 2 and 4 were about 10 percent lower for participants than they would have been in the absence of the program.

Given these findings, it is not surprising that the control group had slightly more arrests on average than the program group (0.8, compared to 0.7). These impacts were due to differences in the arrest rate for the program and control groups and not to differences in the average number of arrests for those arrested (which was 2.3 for both groups). Among those arrested, control group members were also typically arrested sooner after random assignment than program group members (15.0 months, on average, as compared to 16.4 months).

### 2. Impacts on Arrest Charges

We find that Job Corps participation led to a 16 percent reduction in the arrest rate during the 48-month follow-up period. An important policy question is the extent to which these reductions were concentrated in certain types of crimes or were spread uniformly across crime types (that is, the extent to which Job Corps affected the mix of crimes committed by program participants).

To address this issue, we divided crimes into eight categories (Table VII.7) that broadly match crime categories defined by the Bureau of Justice Statistics (BJS). To calculate crime-related social costs as part of the benefit-cost analysis, we rely heavily on data the BJS collected.

We also estimated impacts separately for finer categories of crimes. However, many of these crimes were rare, so the statistical power for detecting true impacts on them is very low. Furthermore, respondents often did not provide sufficient information about their arrest charges to allow for coding to these finer categories. Hence, some finer charges may be misclassified.

## TABLE VII.7

## CRIME CATEGORIES

| Category | Type of Crime |
|---|---|
| Murder | Murder or manslaughter |
| Assault | Aggravated assault, forcible rape, kidnaping, justifiable homicide |
| Robbery | Robbery |
| Burglary | Burglary |
| Larceny, Vehicle Theft, or Other Property Crimes | Arson, embezzlement, forgery or counterfeiting, fraud, larceny or theft, motor vehicle theft or carjacking, shoplifting, buying, receiving, or possessing stolen property, vandalism, blackmail or extortion, bad checks |
| Drug Law Violations | Use or possession of drugs or drug equipment violations, sale or manufacture of drugs |
| Other Personal Crimes | Simple assault, family offenses, sex offenses other than rape, fighting |
| Other Miscellaneous Crimes | Disorderly conduct, liquor-related crimes, gambling, loitering or vagrancy or curfew violations, parole or probation violation, prostitution, weapons offenses, bribery, being a Peeping Tom, trespassing on real property, having an outstanding warrant, pornography, obstruction of justice, motor vehicle violations, smoking cigarettes under age, truancy, being a runaway |

Therefore, we focus our discussion on the impact estimates for the broader crime categories. Table F.1 presents the impact results for the finer categories.[6]

Sample members were most frequently arrested for "miscellaneous" crimes, the most common of which were disorderly conduct, liquor violations, parole violations, obstruction of justice, weapons violations, trespassing, and motor vehicle violations (Tables VII.8 and F.1). Nearly 20 percent of control group members were arrested for these crimes. About 9 percent of control group members were arrested for larceny, vehicle theft, or other property crimes; 8 percent were arrested for drug law violations; and 5 percent were arrested for other personal crimes (simple assault was the most common of these charges). More than 8 percent of control group members were arrested for serious crimes (aggravated assault, murder, robbery, or burglary).

Program group members were less likely to have arrest charges for *all* categories of crimes except for assault, which suggests that crime reductions due to Job Corps participation were spread uniformly across crime types. The reductions for miscellaneous crimes (the most common type) were slightly larger in proportional terms than for the other crime categories. The proportion of participants who were arrested for miscellaneous crimes was about 4 percentage points lower than it would have been in the absence of the program, which translates into a reduction in these crimes of about 20 percent. Job Corps participation also reduced the arrest rate for more serious crimes, although the magnitude of these impacts is smaller and not statistically significant. Job Corps participation led to a reduction of about 15 percent for burglaries and drug law violations, 10 percent for murders, robberies, and larceny, and 5 percent for other personal crimes. As expected, impacts

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[6] We present impact estimates only for crimes that were committed by at least 15 program group members and 15 control group members.

## TABLE VII.8

## IMPACTS ON ARREST CHARGES

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Most Serious Charge for Which Arrested (Percentages) | | | | | | |
| Never arrested | 71.8 | 67.8 | 4.0***[d] | 73.0 | 5.5***[d] | 8.2 |
| Murder | 0.43 | 0.46 | -0.03 | 0.39 | -0.04 | -9.7 |
| Assault | 3.9 | 3.7 | 0.1 | 3.9 | 0.2 | 5.1 |
| Robbery | 1.4 | 1.7 | -0.3 | 1.3 | -0.4 | -23.8 |
| Burglary | 2.0 | 2.3 | -0.3 | 1.7 | -0.5 | -21.6 |
| Larceny, vehicle theft, or other property crimes | 5.8 | 6.5 | -0.7 | 5.7 | -1.0 | -14.2 |
| Drug law violations | 4.5 | 5.5 | -1.0 | 4.2 | -1.3 | -24.2 |
| Other personal crimes | 3.2 | 3.3 | -0.1 | 3.0 | -0.1 | -4.0 |
| Other miscellaneous crimes | 7.0 | 8.7 | -1.7 | 6.9 | -2.4 | -25.7 |
| Percentage Had a Serious Arrest Charge[e] | 7.9 | 8.4 | -0.4 | 7.4 | -0.6 | -7.7 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.43 | 0.46 | -0.03 | 0.39 | -0.04 | -9.7 |
| Assault | 4.1 | 3.8 | 0.3 | 4.1 | 0.4 | 9.5 |
| Robbery | 2.1 | 2.2 | -0.1 | 1.9 | -0.2 | -8.1 |
| Burglary | 2.7 | 3.0 | -0.4 | 2.3 | -0.5 | -17.9 |
| Larceny, vehicle theft, or other property crimes | 8.0 | 8.6 | -0.6 | 7.5 | -0.9 | -10.4 |
| Drug law violations | 7.1 | 7.9 | -0.7 | 6.8 | -1.0 | -13.0 |
| Other personal crimes | 5.2 | 5.4 | -0.2 | 5.3 | -0.3 | -5.1 |
| Other miscellaneous crimes | 16.6 | 19.5 | -2.8*** | 15.6 | -4.0*** | -20.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

[e] Serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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were larger for most crime categories in the first year after random assignment than in later years (Table F.2).

### 3. Impacts on Convictions

Beneficial program impacts on arrest-related outcomes translated into beneficial impacts on conviction-related outcomes (Figure VII.6 and Table VII.9). More than 25 percent of control group members were convicted, pled guilty, or were adjudged delinquent during the 48-month follow-up period, compared to 22 percent of program group members (and 21 percent of Job Corps participants). These impacts were due to differences in the arrest rate by research status and not to differences in the conviction rate among those arrested (because about three-quarters of those arrested were convicted in both groups). The statistically significant impact on the conviction rate for participants was about 4 percentage points--a 17 percent reduction. Similarly, control group members had more convictions on average than program group members (0.43, compared to 0.37).[7]

Job Corps participation reduced convictions for all types of charges except murder and assault, and the pattern of findings closely follows the pattern for the arrest charges. For example, the impacts on conviction charges were largest for those convicted of miscellaneous crimes and were negative but smaller for most other crime types.

There is evidence that conviction charges were less serious than arrest charges. For example, 14.3 percent of control group and 12.2 percent of program group members made a deal or plea-bargained. Furthermore, a higher proportion of youths were arrested for violent crimes than were actually convicted of them.

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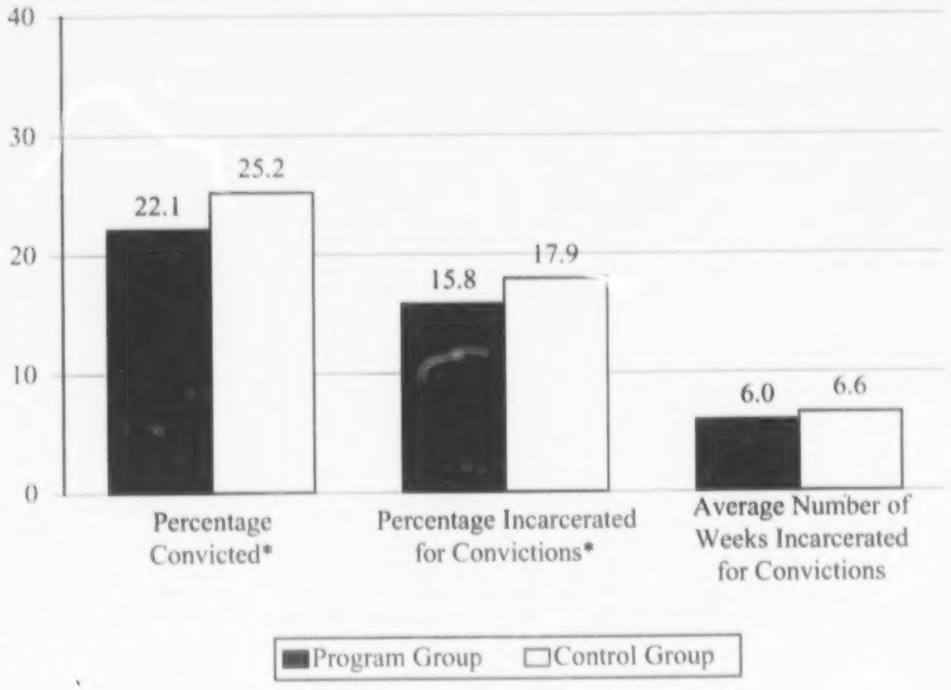
[7] We did not obtain information on the dates that youth were convicted. We examined conviction rates over time by using the arrest date that corresponded to each conviction. These estimates were difficult to interpret, however, because of the lag between arrests and convictions and because of differences in the lag by type of crime. Thus, we do not report these estimates.

FIGURE VII.6

CONVICTIONS AND INCARCERATIONS RESULTING FROM CONVICTIONS DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VII.9

## IMPACTS ON CONVICTION RATES AND CHARGES

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment | 22.1 | 25.2 | -3.1*** | 20.8 | -4.3*** | -17.0 |
| Number of Times Convicted (Percentages) | | | | | | |
| 0 | 78.0 | 75.0 | 3.0***[d] | 79.3 | 4.2***[d] | 5.6 |
| 1 | 12.8 | 14.9 | -2.1 | 12.3 | -2.9 | -19.2 |
| 2 | 5.6 | 6.0 | -0.4 | 5.2 | -0.6 | -10.3 |
| 3 or more | 3.6 | 4.1 | -0.5 | 3.2 | -0.7 | -17.9 |
| Average Number of Times Convicted | 0.37 | 0.43 | -0.05*** | 0.34 | -0.08*** | -18.0 |
| Percentage Made a Deal or Plea-Bargained | 12.2 | 14.3 | -2.1*** | 11.1 | -2.9*** | -20.5 |
| Most Serious Charge for Which Convicted (Percentages) | | | | | | |
| Never convicted | 78.4 | 75.2 | 3.2***[d] | 79.7 | 4.4***[d] | 5.8 |
| Murder | 0.29 | 0.26 | 0.03 | 0.25 | 0.04 | 18.2 |
| Assault | 2.3 | 2.2 | 0.1 | 2.1 | 0.1 | 6.6 |
| Robbery | 1.1 | 1.5 | -0.4 | 0.8 | -0.6 | -43.6 |
| Burglary | 1.6 | 1.8 | -0.2 | 1.4 | -0.3 | -15.3 |
| Larceny, vehicle theft, or other property crimes | 4.7 | 5.0 | -0.3 | 4.6 | -0.5 | -9.2 |
| Drug law violations | 4.1 | 4.9 | -0.8 | 3.7 | -1.2 | -23.7 |
| Other personal crimes | 2.0 | 1.9 | 0.0 | 2.0 | 0.0 | 0.3 |
| Other miscellaneous crimes | 5.7 | 7.2 | -1.5 | 5.4 | -2.1 | -27.7 |
| Percentage Convicted of a Serious Charge[e] | 5.2 | 5.7 | -0.5 | 4.5 | -0.7 | -13.5 |
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.29 | 0.26 | 0.03 | 0.25 | 0.04 | 18.1 |
| Assault | 2.4 | 2.2 | 0.1 | 2.1 | 0.2 | 8.2 |
| Robbery | 1.4 | 1.8 | -0.4* | 1.0 | -0.6* | -37.9 |
| Burglary | 1.7 | 2.1 | -0.3 | 1.6 | -0.5 | -23.0 |
| Larceny, vehicle theft, or other property crimes | 5.8 | 6.0 | -0.2 | 5.6 | -0.3 | -4.8 |
| Drug law violations | 5.5 | 6.0 | -0.5 | 5.0 | -0.7 | -11.7 |
| Other personal crimes | 3.1 | 3.4 | -0.3 | 3.3 | -0.4 | -9.9 |
| Other miscellaneous crimes | 11.3 | 13.0 | -1.7*** | 10.4 | -2.4*** | -18.5 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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TABLE VII.9 *(continued)*

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[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

[e] Serious arrest charges include murder or assault, robbery, or burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

### 4. Impacts on Incarcerations Resulting from Convictions and on Probation and Parole Rates

Job Corps participation also reduced incarceration rates and the time spent incarcerated resulting from convictions (Figure VII.6 and Table VII.10).[8] About 18 percent of control group members were ever incarcerated for convictions, compared to about 16 percent for program group members (a statistically significant impact of 2 percentage points per eligible applicant). The impact per participant was about 3 percentage points (a 17 percent reduction in the incarceration rate). These impacts were due to impacts on the conviction rate and not to differences in the incarceration rate among those convicted (which was about 70 percent for each group). Participants spent an average of 5 weeks in jail but spent an average of about six days (0.8 weeks) less in jail than they would have if they had not enrolled in Job Corps.[9] This impact translates to a 14 percent reduction in time spent in jail during the 48-month follow-up period.

Job Corps also had an effect on the percentage of participants who were put on probation or parole for crimes committed after random assignment. About 14.6 percent of control group members were put on probation or parole, compared to 13.5 percent of program group members (and 12.5 percent of participants). The impact per participant, 1.6 percentage points, is statistically significant at the 10 percent level.

### 5. Subgroup Results

For the analysis of subgroup impacts on crime-related outcomes, we focus on subgroups defined by age, gender, and residential designation status. We hypothesized that crime impacts would differ

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[8] We collected incarceration information for those who were convicted, pled guilty, or were adjudged delinquent. We did not collect incarceration information for those whose arrest charges were dismissed or dropped or who were acquitted.

[9] Incarcerated youth spent an average of about 8.5 months in jail for both research groups.

TABLE VII.10

IMPACTS ON INCARCERATIONS RESULTING FROM CONVICTIONS AND ON PROBATION AND PAROLE RATES

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Served Time in Jail for Convictions During the 48 Months After Random Assignment | 15.8 | 17.9 | -2.1*** | 14.6 | -2.9*** | -16.7 |
| Total Number of Months Ever in Jail for Convictions (Percentages) | | | | | | |
| 0 | 85.3 | 83.4 | 1.9 | 86.4 | 2.7 | 3.2 |
| Less than 1 | 4.5 | 5.6 | -1.1 | 4.5 | -1.5 | -25.5 |
| 1 to 3 | 2.4 | 2.8 | -0.3 | 2.3 | -0.4 | -16.1 |
| 3 to 6 | 1.9 | 1.8 | 0.0 | 1.8 | 0.0 | 2.8 |
| 6 to 12 | 1.8 | 1.9 | -0.1 | 1.7 | -0.1 | -6.7 |
| 12 to 18 | 1.5 | 1.6 | -0.1 | 1.1 | -0.1 | -10.9 |
| 18 to 24 | 1.0 | 1.0 | 0.0 | 0.8 | 0.0 | -1.3 |
| 24 or more | 1.6 | 2.0 | -0.4 | 1.3 | -0.5 | -26.9 |
| Average Time in Jail | | | | | | |
| Months | 1.4 | 1.5 | -0.1 | 1.2 | -0.2 | -13.8 |
| Weeks | 6.0 | 6.6 | -0.6 | 5.0 | -0.8 | -13.8 |
| Weeks for those in jail | 37.4 | 35.8 | 1.5 | 34.2 | 2.1 | 6.7 |
| Percentage Ever Put on Probation or Parole | 13.5 | 14.6 | -1.2* | 12.5 | -1.6* | -11.5 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline, 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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across age and gender subgroups because of differences in their baseline characteristics and, in particular, because of substantial differences in their experiences with the criminal justice system before program application. For example, a higher proportion of younger than older applicants in our sample reported having ever been arrested before program application, and the arrest rate for males was double that of females during the preprogram period. We expected that crime impacts would be larger for residential than nonresidential students, because students living on center would have less opportunity to get into trouble with the law than students who train on center during the day but return home at night.

In this section, we present impact findings on the full set of crime measures for these key subgroups. Then we briefly present impact findings on key crime measures for other subgroups defined by youth characteristics.

### a. Impacts by Age

As expected, the younger sample reported more arrests than the older sample (Figure VII.7 and Tables F.3 to F.5). More than 41 percent of control group members who were 16 and 17 at program application were ever arrested during the 48-month follow-up period, compared to about 30 percent of those 18 and 19, and about 22 percent of those 20 to 24.[10] In addition, arrest rates were higher for the younger applicants in each year (they were about 15 to 18 percent per year for the youngest group and about 5 to 9 percent per year for the oldest group). Furthermore, conviction and incarceration rates resulting from convictions were highest for the youngest group. This same age pattern holds for males and females (not shown).

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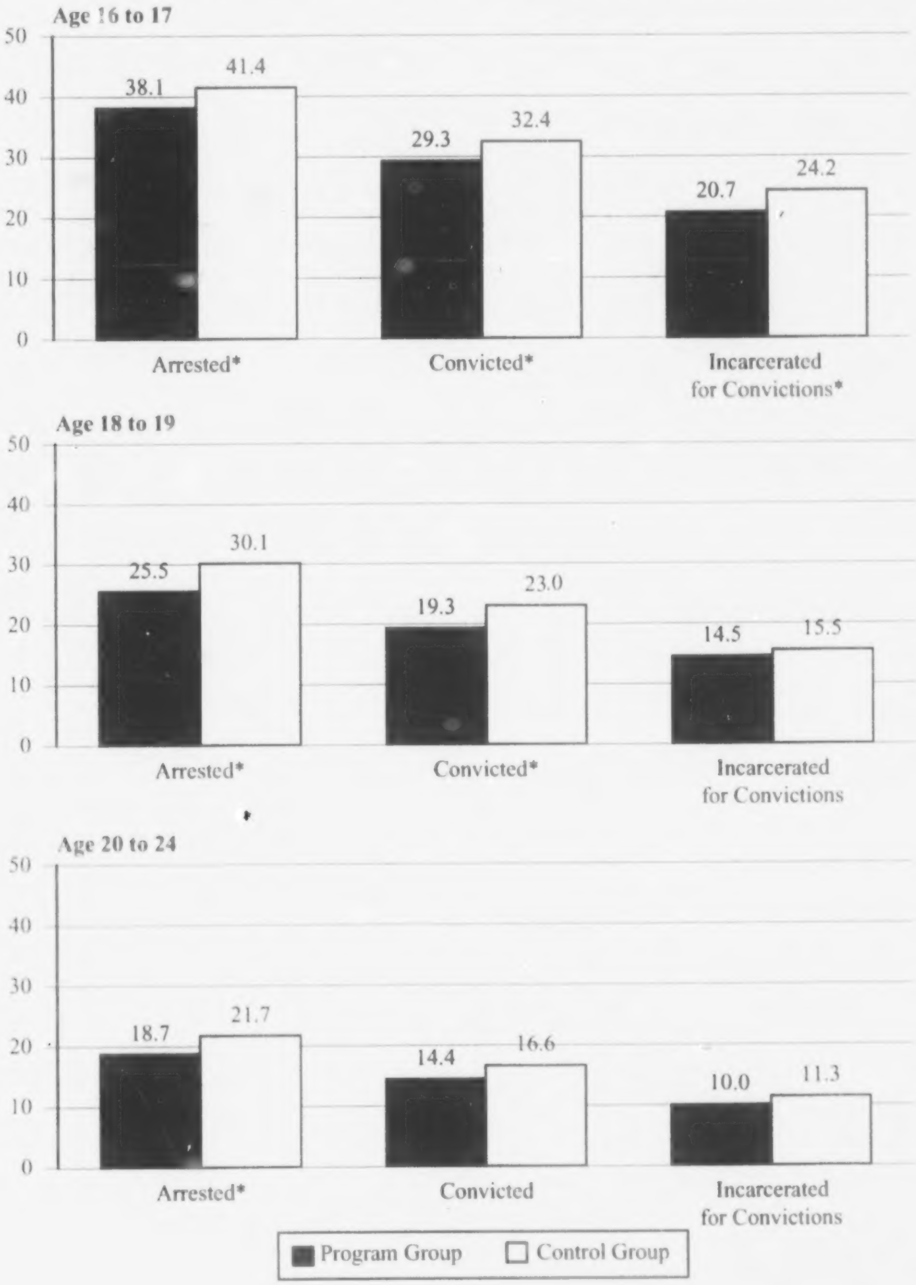
[10] The distribution of arrest charges for those arrested, however, was similar by age.

FIGURE VII.7

PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD, BY AGE



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

These findings are consistent with published statistics that report that criminal activity typically declines as teenagers mature. The findings may also be due to the fact that the younger applicants were somewhat more disadvantaged at baseline (and in particular, had higher reported arrest rates) and thus, may have reported higher crime activity during the follow-up period.

Although the *level* of involvement with the criminal justice system differed by age, the crime *impacts* were very similar. Arrest, conviction, and incarceration rates were significantly higher for the control group than the program group for all three age groups, and the size of the impacts was similar (although the percentage reduction in the crime measures due to program participation was larger for the older groups because of their lower level of criminal activity). In general, impacts on the types of arrest and conviction charges were also similar. These same results hold for males and females.

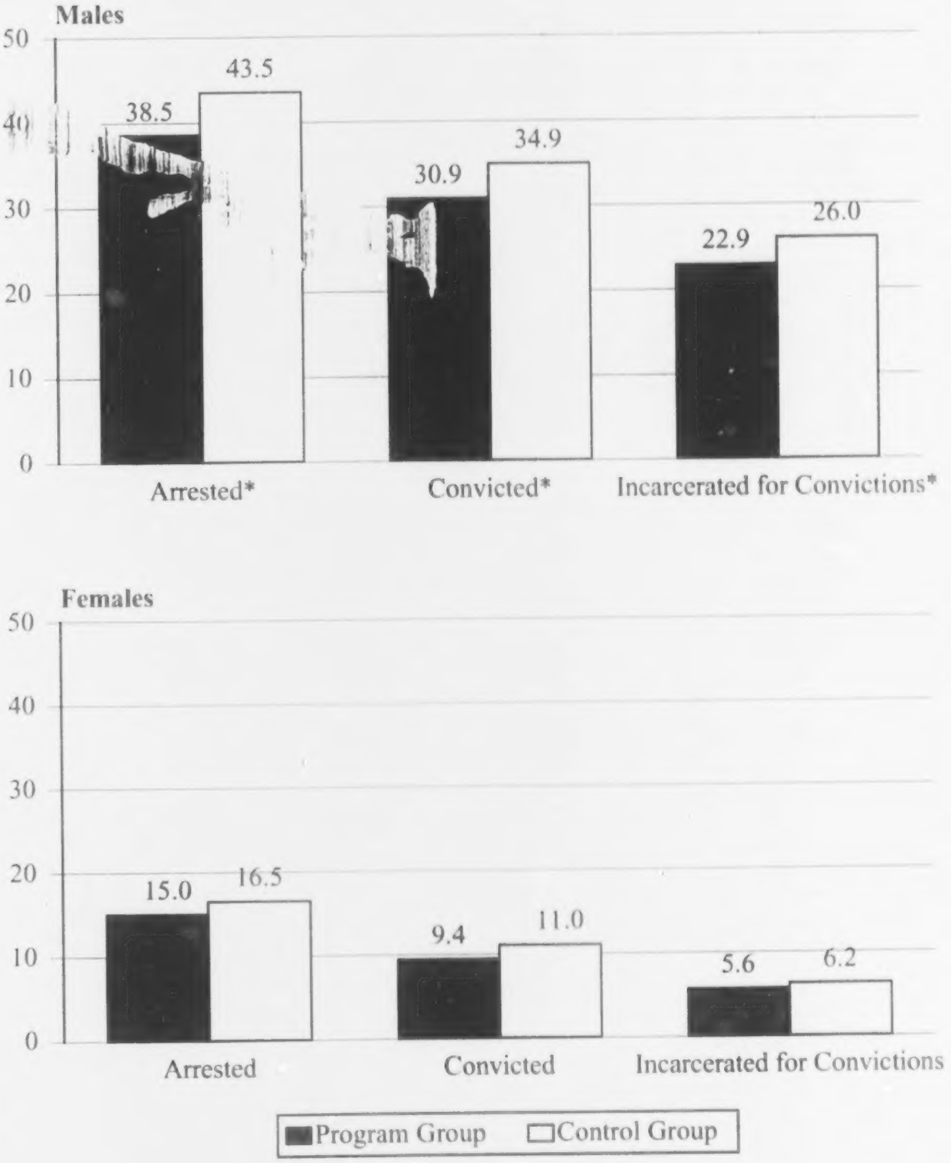
There were also few age differences in the pattern of impacts over time. The arrest reductions were largest in the first year after random assignment for all three age groups. There is some evidence, however, that the arrest reductions in years 2 to 4 were larger for those 16 and 17 than for the older groups.

### b. Impacts by Gender

Not surprisingly, males had much higher arrest, conviction, and incarceration rates than females during the follow-up period (Figure VII.8 and Tables F.6 and F.7). About 44 percent of control group males were ever arrested, compared to only 17 percent of control group females, and the 48-month conviction rate was nearly 35 percent for males but only 11 percent for females. About 26 percent of control group males were incarcerated for convictions, as compared to about a fourth of that for control group females. In addition, among those arrested, males were much more likely than females to have committed serious crimes.

FIGURE VII.8

PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD, BY GENDER



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

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Job Corps participation led to reductions in criminal activity for both males and females, although the impacts were larger for males. The arrest rate was 5 percentage points lower for program group males than control group males (38.5 percent, compared to 43.5 percent), and this impact estimate is statistically significant. The arrest rate was only 1.5 percentage points lower for program group females than control group females (15 percent, compared to 16.5 percent). These impacts translate into 15 percent reductions for both male and female participants. Percentage reductions in convictions and incarcerations for convictions follow the same pattern. The pattern of impacts by year and type of charge did not differ substantially for the two gender groups.

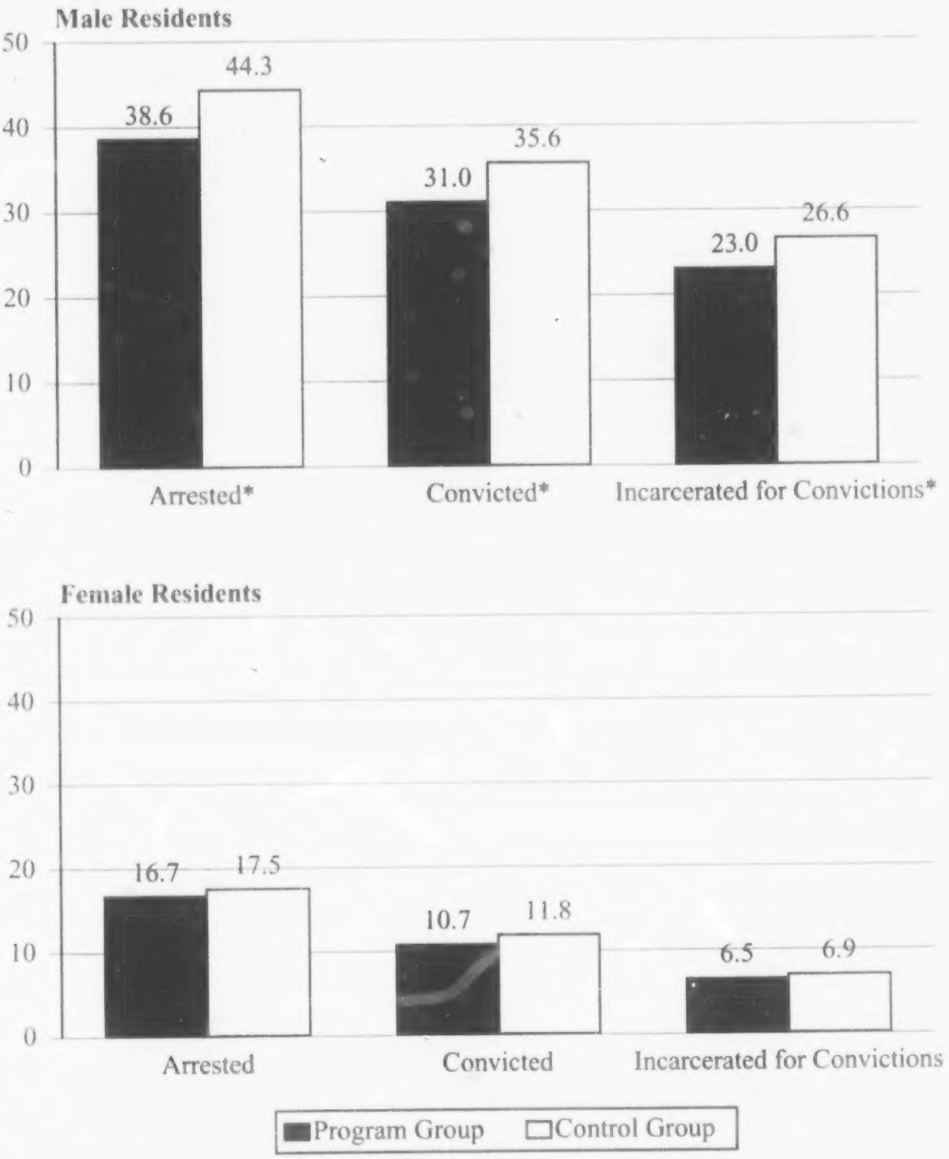
We do find some important differences in the findings for male residents and nonresidents, however, as we discuss next.

### c. Impacts for Residents and Nonresidents

For both males and females, involvement in the criminal justice system was higher for those designated for residential slots than for those designated for nonresidential slots (Figures VII.9 and VII.10 and Tables F.8 to F.11). Among the control group, about 44 percent of male residential designees were arrested during the 48 months after random assignment, compared to 33 percent of male nonresidential designees; the arrest rates for control group females in the two components were 18 and 13 percent, respectively. These findings reflect differences in the characteristics of students who are suitable for the residential and nonresidential components. They are consistent with what one would expect given that residential students are deemed to need training away from their home communities, whereas nonresidential students are not.

FIGURE VII.9

PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD FOR RESIDENTIAL DESIGNEES, BY GENDER

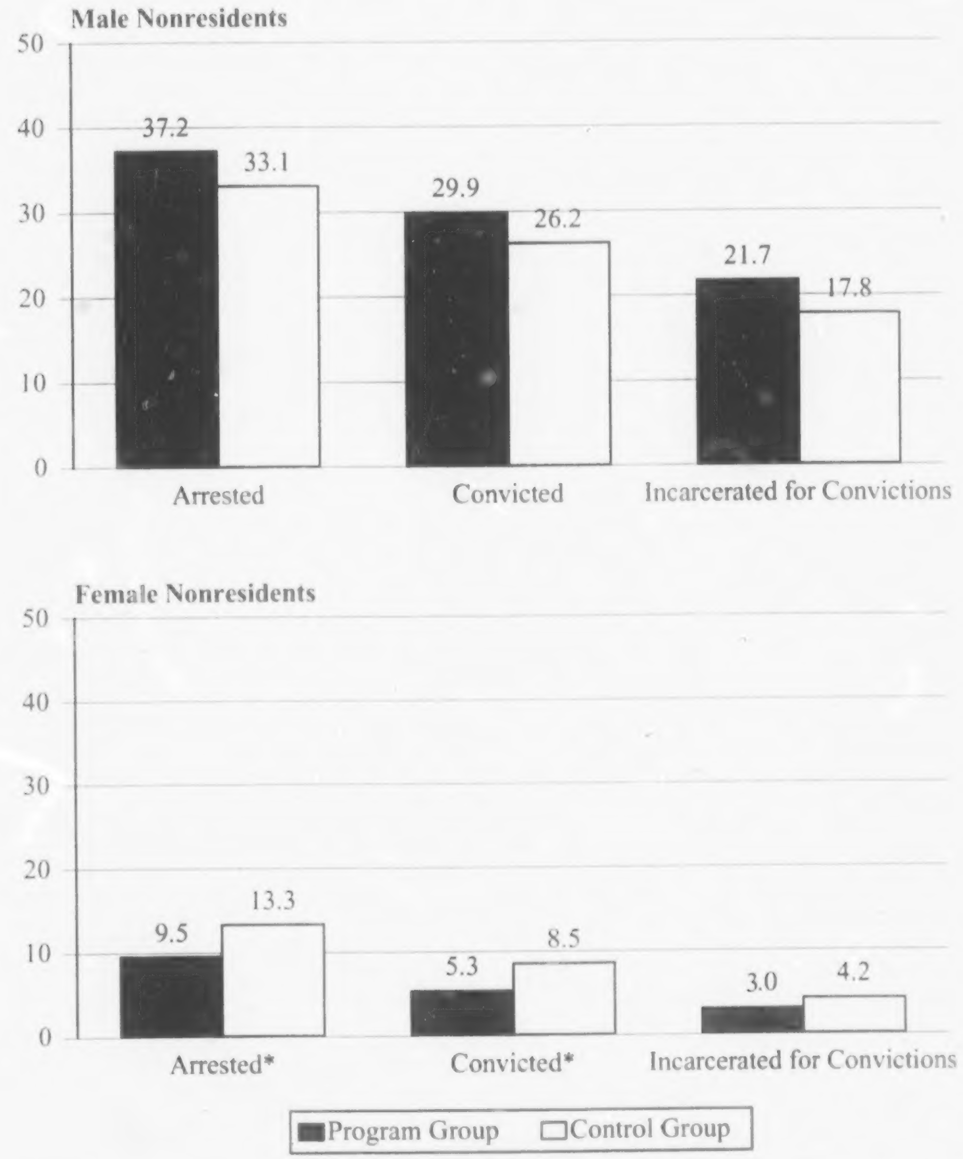


Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

FIGURE VII.10

PERCENTAGE EVER ARRESTED, CONVICTED, AND INCARCERATED FOR CONVICTIONS DURING THE 48-MONTH PERIOD FOR NONRESIDENTIAL DESIGNEES, BY GENDER



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

Participation in the *residential* component led to reductions in criminal activity for both males and females, although the effects were larger for males. About 44.3 percent of control group males designated for residential slots were ever arrested, compared to 38.6 percent of program group males designated for residential slots (a statistically significant impact of about 6 percentage points per eligible applicant). These arrest rate reductions were largest during the first year after random assignment, but they did persist afterwards. The impact on the 48-month arrest rate for residential females was -0.8 percentage points (16.7 percent for the program group and 17.5 percent for the control group), although this small impact is not statistically significant. These findings suggest that removing disadvantaged youths from their home environments into a residential program for a significant period of time can reduce their involvement with the criminal justice system both while they are enrolled and afterwards.

Criminal involvement was reduced for females designated for *nonresidential* slots, but *not* for males designated for nonresidential slots. Impacts on the 48-month arrest and conviction rates were statistically significant for female nonresidential designees, and were larger than those for female residential designees. Arrest, conviction, and incarceration rates for male nonresidential designees, however, were actually larger for the program group than the control group, although these impacts are not statistically significant. Moreover, impacts on six of the eight arrest charge categories were positive for the male nonresidents (although none are statistically significant at the 5 percent level).

We emphasize again that our results for males do not necessarily imply that males in the nonresidential component would have better average crime outcomes if they were instead assigned to the residential component. As discussed, differences between the characteristics of males assigned to each component could lead to misleading conclusions about how each group would fare in the other component.

### d. Impacts for Other Subgroups

Job Corps reduced involvement with the criminal justice system during the 48-month period for nearly all other key subgroups defined by youth characteristics (Table F.12). Impacts were similar for females with and without children at baseline, by race and ethnicity, and for those with and without a high school credential at baseline (despite the fact that the arrest rate was nearly twice as high for those without a credential). Job Corps reduced criminal activities for those who reported having been arrested prior to random assignment and for those who did not (although the arrest rate was about 50 percent for the arrested group). None of the differences in the impacts across levels of these subgroups are statistically significant.

Finally, impacts on convictions and incarcerations were somewhat larger for the post-ZT group than for the pre-ZT group. These results, however, should be interpreted with caution, for two reasons. First, the pre-ZT group measures are contaminated, because program group enrollees in this group spent about 78 percent of their total time in Job Corps after the ZT policies took effect. Second, differences in the impact estimates were due partly to lower crime rates for the control group in the pre-ZT group (which is contrary to expectations, because the ZT policies would be thought to discourage those with arrest histories from applying to the program or make them ineligible).

## C. CRIMES COMMITTED AGAINST JOB CORPS PARTICIPANTS

Job Corps participation is expected to lead to reductions in crimes committed *against* program participants. Many Job Corps students come from neighborhoods where crime rates are high, whereas violence is not permitted in Job Corps. Thus, living at a Job Corps center may be physically safer for participants than continuing to live in their neighborhoods, as fewer opportunities arise for students to be victims of crimes. In addition, if Job Corps students relocate to safer neighborhoods or are less idle after leaving Job Corps, the incidence and severity of crimes committed against Job

Corps participants may also be lower after the students leave the program. In the benefit-cost analysis, impacts on crimes committed against participants are valued as program benefits to participants.

This section presents impacts on self-reported crimes committed against sample members for the full sample and for key youth subgroups. We did not obtain information on *each* criminal incident committed against sample members. Instead, we obtained information on the *number of times* each youth was a victim of the following five categories of crimes during the year prior to each follow-up interview: (1) assault; (2) burglary; (3) robbery; (4) car theft; and (5) larceny (pocket picking, purse snatching, money extortion, and theft from or damage to motor vehicles). We also obtained information on the total number of times that the youth was victimized and, because there can be more than one type of victimization during a criminal incident, the number of separate criminal incidents. In addition, we obtained data on the total amount of money that a sample member lost from crimes committed against him or her.

As we discuss next, Job Corps led to reductions in crimes committed against program participants. The frequency of victimizations was reduced most during the in-program period, but the reductions persisted somewhat afterwards. Reductions were found for almost every crime type, and across most subgroups. Our results suggest that Job Corps students are safer in centers than at home.

### 1. Impacts on Victimization Rates

Many control group members were victims of crimes (Table VII.11). Furthermore, the frequency of victimizations among the control group decreased only slightly over time. About 24 percent of control group members were ever victimized in the year prior to the 12-month interview, as compared to about 22 percent in the year prior to the 30-month interview and 18 percent in the year prior to the 48-month interview. The average number of crimes committed against the control group decreased from 0.6 to 0.5 to 0.4 during this same period.

Job Corps participation reduced the percentage who were ever a victim of a crime during the first 12 months after random assignment (when many program group members were enrolled in Job Corps). About 24 percent of control group members reported being the victim of a crime during this period, as compared to 22 percent of program group members (and 21 percent of Job Corps participants). This statistically significant 2 percentage point reduction per eligible applicant translates into a 3 percentage point reduction per participant.

Estimates of impacts on the number of incidents with a criminal victimization during the 12-month period show a similar pattern. Job Corps reduced the average number of crimes against participants by 162 incidents per thousand--a 27 percent reduction. This impact is statistically significant at the 1 percent level. A person can be the victim of more than one crime per incident (referred to as a victimization). Accordingly, we also estimated the impact on the total number of victimizations, which was about 127 per thousand. These findings suggest that Job Corps participants are safer on center than at home.

Reductions in crimes committed against participants persisted during the 30- and 48-month periods but became smaller (Table VII.11). The reduction in the percentage who were ever a victim was about 3 percentage points per participant during the year prior to the 30-month interview, and

## TABLE VII.11

## IMPACTS ON CRIMES COMMITTED AGAINST PARTICIPANTS IN THE PREVIOUS YEAR

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Ever a Victim of a Crime | | | | | | |
| At 12 months | 21.9 | 24.2 | -2.3*** | 21.1 | -3.2*** | -12.9 |
| At 30 months | 19.7 | 22.0 | -2.3*** | 19.7 | -3.2*** | -13.9 |
| At 48 months | 16.6 | 18.2 | -1.6** | 16.7 | -2.2** | -11.6 |
| Average Number of Incidents with a Victimization | | | | | | |
| At 12 months | 0.43 | 0.55 | -0.12*** | 0.43 | -0.16*** | -27.5 |
| At 30 months | 0.44 | 0.47 | -0.03 | 0.48 | -0.04 | -7.1 |
| At 48 months | 0.41 | 0.42 | -0.01 | 0.39 | -0.02 | -3.7 |
| Average Number of Victimizations | | | | | | |
| At 12 months | 0.52 | 0.62 | -0.09** | 0.51 | -0.13** | -20.1 |
| At 30 months | 0.47 | 0.52 | -0.05 | 0.48 | -0.07 | -12.5 |
| At 48 months | 0.40 | 0.42 | -0.03 | 0.39 | -0.04 | -8.2 |
| Average Amount of Money Lost from Victimizations (Dollars) | | | | | | |
| At 12 months | 109.6 | 130.5 | -20.9 | 99.8 | -29.1 | -22.5 |
| At 30 months | 131.1 | 186.1 | -55.1*** | 131.0 | -76.6*** | -36.9 |
| At 48 months | 151.6 | 143.9 | 7.7 | 157.9 | 10.7 | 7.3 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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was about 2 percentage points during the year prior to the 48-month interview. Job Corps reduced the average number of victimizations against participants by 70 per thousand at 30 months and 40 per thousand at 48 months, although these impact estimates are not statistically significant.

Consistent with the finding that control group members were victimized more often than program group members, we find that the average amount of money lost from victimizations was slightly larger for the control group at 12 and 30 months, but not at 48 months (Table VII.11). Control group members lost an average of $21 more in the year prior to the 12-month interview, and $55 more in the year prior to the 30-month interview.

### 2. Impacts on Victimizations by Type of Crime

Assault and larceny were the most common types of crimes against control group members reported at each interview, although the percentages who were the victim of a burglary and robbery were only slightly smaller (Table VII.12). The victimization rates for assault and larceny were about 10 percent each at 12 months, 8 percent each at 30 months, and 6.5 percent each at 48 months. Victimization rates for burglary and robbery decreased from about 6 to 5 to 4 percent over the same period. About 2 percent had their car stolen during the year prior to each interview.

Job Corps participation reduced victimization rates for *every* type of crime at 12 months (Tables VII.12 and G.1). Reductions in the frequency of victimizations were largest for burglary (26 per thousand), robbery (26 per thousand), and larceny (54 per thousand), and these estimated impacts are each statistically significant at the 10 percent level. Reductions at 12 months were smaller for assault and motor vehicle theft. Reductions in the frequency of burglaries and robberies were also statistically significant at 30 months. However, estimated impacts on the frequency of other types of victimizations at 30 months and on all types of victimizations at 48 months were small and not statistically significant.

## TABLE VII.12

## IMPACTS ON VICTIMIZATION RATES IN THE PREVIOUS YEAR, BY CRIME TYPE
(Percentages)

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Ever Victimized at 12 Months, by Crime Type | | | | | | |
| Assault | 9.4 | 9.9 | -0.5 | 9.0 | -0.6 | -6.7 |
| Burglary | 3.8 | 4.9 | -1.1*** | 3.3 | -1.5*** | -31.5 |
| Robbery | 5.8 | 6.6 | -0.8 | 5.7 | -1.1 | -15.6 |
| Larceny[d] | 8.1 | 9.3 | -1.2** | 8.1 | -1.7** | -17.3 |
| Motor vehicle theft | 1.6 | 2.1 | -0.5* | 1.5 | -0.7* | -31.2 |
| Ever Victimized at 30 Months, by Crime Type | | | | | | |
| Assault | 7.7 | 8.5 | -0.8 | 7.6 | -1.1 | -12.7 |
| Burglary | 3.8 | 5.4 | -1.7*** | 3.4 | -2.3*** | -40.6 |
| Robbery | 4.1 | 5.6 | -1.5*** | 4.3 | -2.1*** | -33.2 |
| Larceny[d] | 7.5 | 7.5 | 0.0 | 7.7 | 0.0 | -0.5 |
| Motor vehicle theft | 2.4 | 2.2 | 0.2 | 2.3 | 0.2 | 11.7 |
| Ever Victimized at 48 Months, by Crime Type | | | | | | |
| Assault | 6.4 | 6.2 | 0.1 | 6.5 | 0.2 | 2.9 |
| Burglary | 3.7 | 3.4 | 0.2 | 3.6 | 0.3 | 10.2 |
| Robbery | 3.7 | 4.3 | -0.5 | 4.0 | -0.7 | -15.5 |
| Larceny[d] | 6.0 | 7.1 | -1.1** | 5.9 | -1.5** | -20.5 |
| Motor vehicle theft | 2.1 | 2.4 | -0.4 | 2.0 | -0.5 | -21.2 |
| **Sample Size** | | | | | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

[d] Larceny includes pickpocketing, purse snatching, extortion, and theft from or damage to motor vehicles.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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### 3. Subgroup Results

Job Corps led to reductions in crimes committed against participants during the first 12 months after random assignment for nearly all key subgroups defined by youth characteristics (Table G.2). The impacts on the average number of criminal incidents against participants at 12 months were negative for 18 of the 20 subgroups that we examined. The impacts, however, were somewhat larger for females, those 18 and older, and those with a high school credential than for their counterparts. Importantly, the estimated impacts were similar for residential and nonresidential designees, and for those who applied to Job Corps before and after the ZT policies took effect. Reductions in victimizations were smaller at 30 and 48 months across most subgroups. Thus, it appears that Job Corps leads to reductions in victimizations for most groups served by the program during the period when students are enrolled in it.

## D. TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE, HEALTH, AND MORTALITY

Job Corps may reduce participants' drug and alcohol use, both during and after the program. Reductions in the use of drugs and alcohol are expected while youths are enrolled in the program, because Job Corps forbids the use of these substances at centers and because behavior is closely monitored. When students first arrive on center, they are required to take a drug test, and those who test positive are given 45 days to become drug free. Even after the 45-day period, all students are subject to drug testing if they are suspected of using drugs. Students who are found not to be drug free after the 45-day probationary period are terminated from the program.[11] Because many students test positive for drugs upon enrollment, and because most students stay in the program for an extended period, students may be less likely to use illegal drugs while enrolled than they would otherwise.

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[11] At the time program group members were enrolled in Job Corps, the probationary period was 30 days, not 45 days.

Job Corps also provides some alcohol and drug treatment. If students test positive, they must attend the alcohol and other drugs of abuse (AODA) program. Other students may participate voluntarily. As discussed in Chapter IV, nearly one-half of program group enrollees attended the AODA program, which covers the Job Corps ZT policy, anger control, self-esteem building, and other topics that teach students about decision making. The AODA program may change student attitudes about drug use and provide students with tools to stay off drugs. These factors could lead to reductions in the use of drugs both while students are enrolled in the program and afterwards. Because of the AODA program, participation in Job Corps might also reduce the use of drug treatment programs outside Job Corps.[12]

Job Corps is also expected to improve participants' overall health status, because it offers comprehensive health services and health education. All students are required to submit to a medical examination, including a blood test for HIV, within two weeks of arrival on center. Centers offer basic medical services to students, including routine medical, dental, and mental health care; daily sick call; and any necessary specialist referrals and consultations. We found from our site visits to centers that many youths did not have access to these types of health care prior to enrollment. Thus, students probably receive better health care on center than they would otherwise, which could improve health during both the in-program and the postprogram periods.

Because Job Corps offers health education, it may also improve participants' health in both the short and the long term. Chapter IV showed that about three-quarters of students in the program group took health education classes, which include units on emotional and social well-being, human sexuality, sexually transmitted diseases, nutrition, fitness, dental hygiene, consumer health, and safety. These classes are designed specifically to increase participants' awareness of health issues and instill attitudes conducive to healthful behavior.

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[12] Possible savings to society due to reductions in the use of alcohol and drug treatment programs are calculated as part of the benefit-cost analysis.

Most youths eligible for Job Corps are in good health, because eligibility requires that an applicant be free of serious medical problems. The baseline interview data reveal that about 85 percent of sample members reported being in good or excellent health (Schochet 1998a). Thus, we expect small impacts on overall health outcomes.

Finally, Job Corps may reduce mortality because the program aims to improve the health and other life circumstances of participants. Furthermore, it may reduce fatal crimes committed against participants.

This section presents impacts on self-reported (1) tobacco, alcohol, and illegal drug use; (2) time spent in drug or alcohol treatment outside Job Corps; and (3) health status. For the measures of tobacco, alcohol, and illegal drug use, we used self-reported data on the extent to which sample members used these substances in the 30 days prior to the 12-, 30-, and 48-month interviews. For the drug and alcohol treatment measures, we used information on dates of treatment and the types of treatment programs that were attended. For the health outcomes, we used self-reported information on whether the youth's health was excellent, good, fair, or poor at the 12-, 30-, and 48-month interviews; whether the youth had a serious physical or emotional problem that limited the amount of work that could be done; and, if so, the nature and duration of the problem.

Next, we discuss impact findings for the full sample. Then we present impact findings for key youth subgroups. Appendix H contains supplementary tables.

### 1. Impacts on Tobacco Use

Job Corps had no effect on cigarette smoking (Figure VII.11 and Table VII.13). About half of both the control and program groups smoked cigarettes in the month prior to the 12-month interview, although the percentage was slightly larger for the program group. About half of both groups also smoked cigarettes at 30 and 48 months. Most smokers smoked regularly (Tables H.1 and H.2).

### 2. Impacts on Alcohol Use

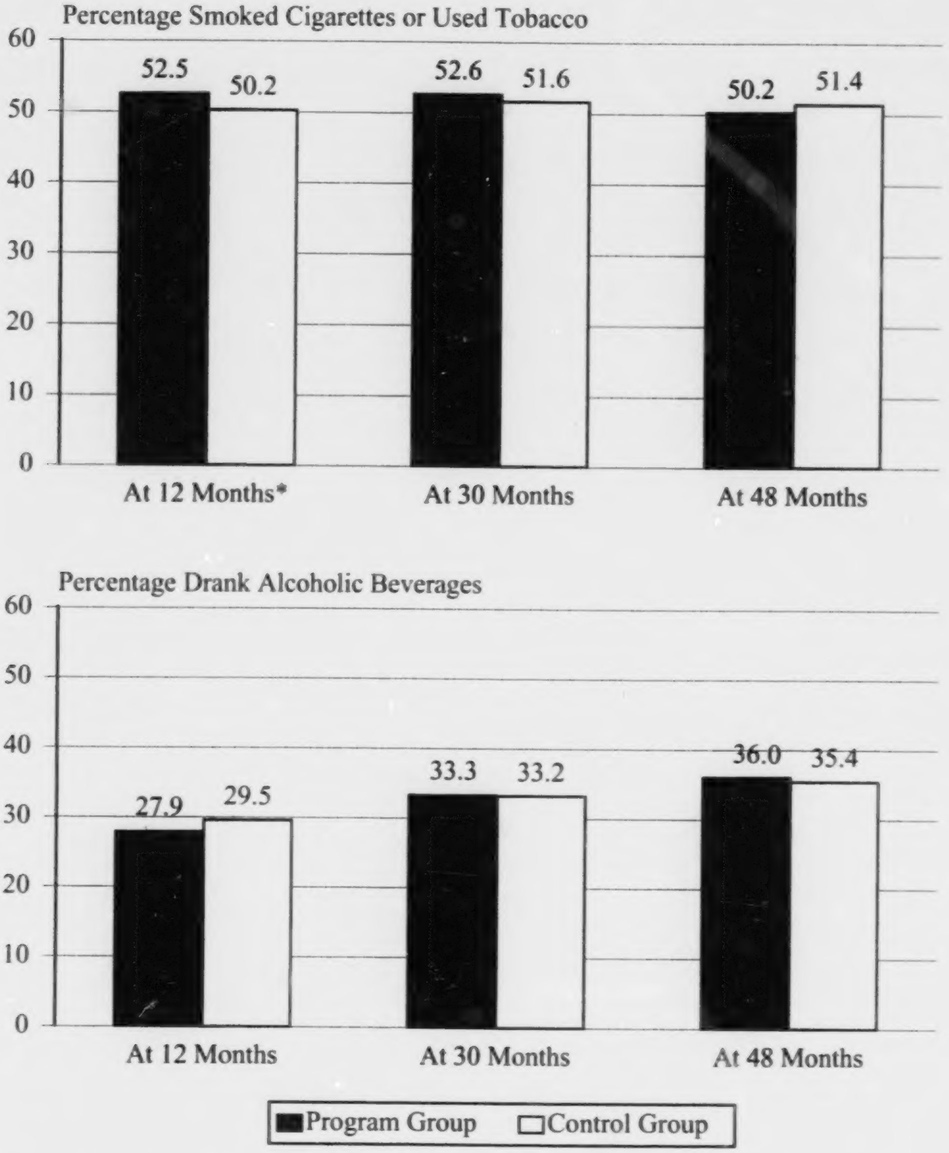
Participation in Job Corps slightly reduced the consumption of alcoholic beverages at 12 months but not at 30 or 48 months (Figure VII.11 and Table VII.13). These findings suggest that alcohol use is reduced while youth are enrolled in Job Corps or soon after they leave, but that reductions do not persist afterwards. About 30 percent of control group members drank alcoholic beverages in the month prior to the 12-month interview, compared to about 28 percent of program group members (an impact of -2 percentage points per eligible applicant). This impact translates to a 7.6 percent reduction due to program participation. The percentage who used alcohol increased to about one-third for each group at 30 months and to about 36 percent for each group at 48 months. About half of those who drank at 48 months did so at least once per week (Tables H.1 and H.2).

### 3. Impacts on Illegal Drug Use

We find no impacts on the reported use of illegal drugs at the 12-, 30-, or 48-month interview points (Figure VII.12 and Table VII.13). About 10 percent of each research group reported using any drugs (marijuana, hashish, or hard drugs) in the month prior to the 12-month interview, 9.9 percent of the program group and 9.5 percent of the control group, a difference which is not statistically significant. About 8.7 percent reported using any drugs in the month prior to the 30-

FIGURE VII.11

TOBACCO AND ALCOHOL USE IN THE 30 DAYS PRIOR TO THE
12-, 30-, AND 48-MONTH INTERVIEWS



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VII.13

## TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 12-, 30-, AND 48-MONTH FOLLOW-UP INTERVIEWS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Smoked Cigarettes** | | | | | | |
| At 12 months | 52.5 | 50.2 | 2.2** | 53.4 | 3.1** | 6.2 |
| At 30 months | 52.6 | 51.6 | 1.0 | 53.1 | 1.4 | 2.7 |
| At 48 months | 50.2 | 51.4 | -1.2 | 50.0 | -1.7 | -3.3 |
| **Consumed Alcoholic Beverages** | | | | | | |
| At 12 months | 27.9 | 29.5 | -1.6* | 27.3 | -2.2* | -7.6 |
| At 30 months | 33.3 | 33.2 | 0.1 | 33.5 | 0.1 | 0.3 |
| At 48 months | 36.0 | 35.4 | 0.6 | 36.4 | 0.8 | 2.2 |
| **Used Marijuana, Hashish, or Hard Drugs** | | | | | | |
| At 12 months | 9.9 | 9.5 | 0.4 | 10.3 | 0.6 | 6.3 |
| At 30 months | 8.7 | 8.8 | -0.1 | 9.2 | -0.1 | -1.2 |
| At 48 months | 7.4 | 7.7 | -0.3 | 7.5 | -0.4 | -4.8 |
| **Used Marijuana or Hashish** | | | | | | |
| At 12 months | 9.5 | 8.9 | 0.6 | 9.9 | 0.8 | 8.9 |
| At 30 months | 8.2 | 8.4 | -0.2 | 8.8 | -0.3 | -3.2 |
| At 48 months | 7.1 | 7.3 | -0.2 | 7.2 | -0.2 | -3.0 |
| **Used Hard Drugs** | | | | | | |
| At 12 months | 1.8 | 1.7 | 0.2 | 1.8 | 0.2 | 12.8 |
| At 30 months | 1.8 | 1.7 | 0.1 | 1.8 | 0.1 | 7.5 |
| At 48 months | 1.8 | 1.6 | 0.2 | 1.8 | 0.2 | 13.2 |
| **Snorted Cocaine Powder** | | | | | | |
| At 12 months | 0.4 | 0.2 | 0.2* | 0.4 | 0.3* | 241.0 |
| At 30 months | 0.3 | 0.4 | 0.0 | 0.4 | -0.1 | -13.2 |
| At 48 months | 0.3 | 0.2 | 0.2* | 0.3 | 0.2* | 405.7 |
| **Smoked Crack Cocaine or Freebased** | | | | | | |
| At 12 months | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | -28.2 |
| At 30 months | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | 29.0 |
| At 48 months | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | -19.0 |
| **Used Speed, Uppers, or Methamphetamines** | | | | | | |
| At 12 months | 0.5 | 0.6 | 0.0 | 0.6 | 0.0 | -6.1 |
| At 30 months | 0.6 | 0.6 | -0.1 | 0.6 | -0.1 | -12.5 |
| At 48 months | 0.3 | 0.5 | -0.1 | 0.2 | -0.2 | -44.2 |
| **Used Hallucinogenic Drugs** | | | | | | |
| At 12 months | 0.9 | 1.0 | -0.1 | 1.0 | -0.1 | -8.0 |
| At 30 months | 0.6 | 0.6 | 0.0 | 0.7 | 0.0 | -1.0 |
| At 48 months | 0.3 | 0.7 | -0.4*** | 0.3 | -0.5*** | -59.1 |
| **Used Heroin, Opium, Methadone, or Downers** | | | | | | |
| At 12 months | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 246.9 |
| At 30 months | 0.2 | 0.2 | -0.1 | 0.1 | -0.1 | -46.3 |
| At 48 months | 0.1 | 0.2 | 0.0 | 0.2 | 0.0 | -3.0 |

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TABLE VII.13 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Used Other Drugs | | | | | | |
| At 12 months | 0.3 | 0.3 | 0.0 | 0.2 | 0.0 | -4.4 |
| At 30 months | 0.1 | 0.1 | 0.0 | 0.2 | 0.0 | 31.1 |
| At 48 months | 0.1 | 0.2 | 0.0 | 0.2 | 0.0 | -21.2 |
| Shot or Injected Drugs with a Needle or Syringe | | | | | | |
| At 12 months | 0.0 | 0.1 | -0.1 | 0.0 | -0.1 | -78.1 |
| At 30 months | 0.1 | 0.1 | 0.0 | 0.0 | 0.0 | -23.3 |
| At 48 months | 0.0 | 0.2 | -0.1** | 0.0 | -0.2** | -100.5 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

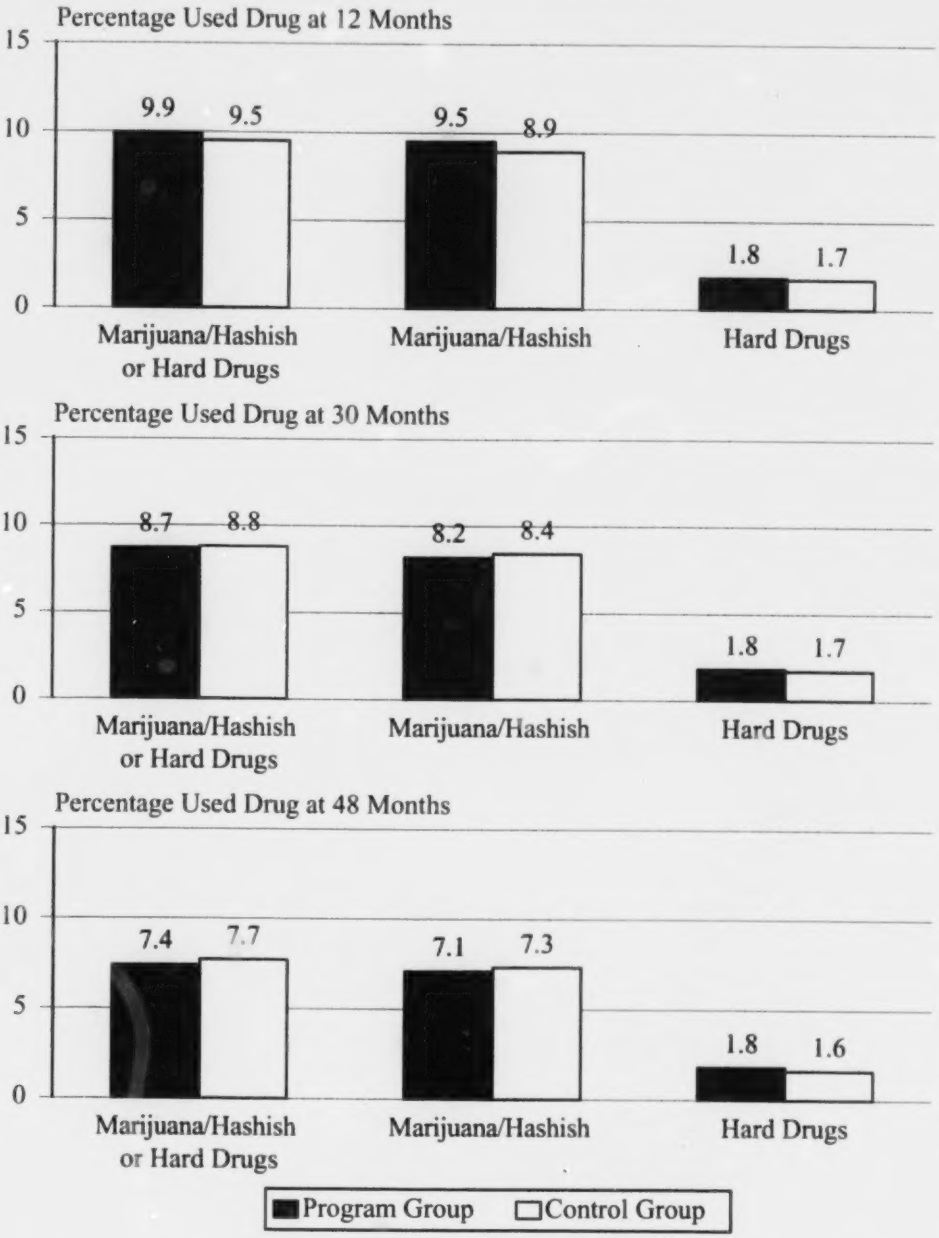
[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

FIGURE VII.12

ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 12-, 30-, AND 48-MONTH INTERVIEWS



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

month interview, and 7.4 percent in the month prior to the 48-month interview. Most drug users reported using marijuana or hashish only; less than 2 percent reported using hard drugs at each interview, including cocaine (about 0.3 percent); crack (about 0.1 percent); speed, uppers, or methamphetamines (about 0.5 percent); hallucinogens (about 0.7 percent); and heroin, opium methadone, or downers (about 0.1 percent). The 12-, 30-, and 48-month impacts for nearly all types of drugs are not statistically significant at the 5 percent level.

Impact estimates on illegal drug use should be interpreted with caution, because of the likely underreporting of drug use. Job Corps program records indicate that 33.6 percent of enrollees in 1995 tested positive (from a urine test) for drugs at enrollment, whereas less than 10 percent of sample members reported at the 12-month interview that they used drugs in the past 30 days. Furthermore, rates of drug use for each type of drug were much higher using the program data than the survey data. For example, about 33 percent used marijuana according to the program data, compared to about 9 percent according to the survey data. Similarly, the program data indicate that 1.3 percent used cocaine, whereas about 0.3 percent reported using cocaine at 12 months. To be sure, the rates of drug use might have been greater at program enrollment than at the 12-month interview. However, the large differences in the levels of drug use from the two data sources strongly suggest that the self-reported measures are too low.[13,14]

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[13] Extensive methodological work on collecting data on illegal drug use has shown that collecting such data through telephone interviews leads to misreporting. Indeed, major national studies designed to measure drug use, such as the National Household Survey of Drug Use, use in-person data collection methods that allow respondents to answer questions about drug use without the interviewer (or anyone else) knowing what the response was. Use of these methods was not feasible for the National Job Corps Study, given that most data were collected through telephone interviews.

[14] We also compared the program data to self-reported drug use measures from the baseline interview because these data were obtained at roughly the same time (see Schochet [1998a], which displays the baseline interview measures). Although these two sets of drug use measures are similar, they are not directly comparable. The baseline interview data contain information on drug use in the

(continued...)

This underreporting, however, does *not* necessarily imply that the estimated impacts on the drug use measures are seriously biased. This is because both program and control group members probably underreported their drug use. The extent of the bias in the impact estimates depends on the (unknown) differences in the amount and nature of underreporting for the two research groups. In fact, if the underreporting rates were similar for the program and control groups, then survey-based estimated impacts *relative to the control group mean* (that is, the percentage gain from participation) would be unbiased, even though the impact estimates would be downwardly biased.[15] Thus, our results should be interpreted with caution but should not be discarded.

### 4. Impacts on Drug or Alcohol Treatment

Job Corps participation led to very small reductions in participation in drug or alcohol treatment programs outside Job Corps (Table VII.14). About 7.7 percent of control group members were ever in a treatment program during the 48 months after random assignment, compared to 7.3 percent of

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[14](...continued)
*past year* (not the past 30 days), whereas the program data contain information on recent drug use. The prevalence of drug use is clearly higher over a longer period than a shorter period. Furthermore, interview respondents may be more likely to admit the use of drugs taken in the past than more recently. Thus, drug use rates calculated using the baseline interview data are probably larger than they would have been if we had asked about recent drug use at baseline.

[15] To illustrate, the impact on a self-reported drug use measure $I$ can be written as follows:

$$(1)\quad I = D_p\,(1-U_p) - D_c\,(1-U_c),$$

where $D_p$ is the *true* percentage of program group members who used the drug, $U_p$ is the rate of underreporting for the program group, and similarly for the control group. If the rate of underreporting was similar by research status (and denoted by $U$), then the impact in equation (1) reduces to $(D_p-D_c)(1-U)$, and the control group mean would be $D_c(1-U)$. In this case, the survey-based estimated impact relative to the control group mean would be $(D_p-D_c)/D_c$, which is an unbiased estimate. If the rates of underreporting differed substantially by research status, then this result does not hold, because the rates of underreporting would not cancel from both the numerator and the denominator.

## TABLE VII.14

## IMPACTS ON PARTICIPATION IN DRUG OR ALCOHOL TREATMENT PROGRAMS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage in a Drug or Alcohol Treatment Program, by Year | | | | | | |
| All years | 7.3 | 7.7 | -0.3 | 6.9 | -0.5 | -6.4 |
| 1 | 2.2 | 2.5 | -0.3 | 2.2 | -0.4 | -14.1 |
| 2 | 2.8 | 3.0 | -0.3 | 2.5 | -0.4 | -12.6 |
| 3 | 2.6 | 2.9 | -0.2 | 2.4 | -0.3 | -12.2 |
| 4 | 2.9 | 2.5 | 0.4 | 2.6 | 0.5 | 25.0 |
| Average Number of Weeks in a Drug or Alcohol Treatment Program, by Year | | | | | | |
| All years | 1.3 | 1.3 | 0.0 | 1.1 | -0.1 | -5.2 |
| 1 | 0.3 | 0.3 | 0.0 | 0.2 | -0.1 | -21.3 |
| 2 | 0.3 | 0.3 | 0.0 | 0.3 | 0.0 | 11.2 |
| 3 | 0.4 | 0.4 | -0.1 | 0.3 | -0.1 | -22.7 |
| 4 | 0.4 | 0.3 | 0.0 | 0.3 | 0.0 | 7.1 |
| Place Where Treatment Was Received | | | | | | |
| Hospital | 0.7 | 0.8 | -0.1 | 0.6 | -0.1 | -16.5 |
| Detoxification center | 0.7 | 0.5 | 0.2 | 0.6 | 0.1 | 54.8 |
| Short-term residential program | 1.6 | 1.8 | -0.3 | 1.4 | -0.4 | -22.3 |
| Long-term residential program | 0.7 | 0.8 | -0.2 | 0.5 | -0.2 | -32.3 |
| Outpatient program | 2.2 | 2.3 | -0.1 | 2.2 | -0.1 | -4.0 |
| Other | 2.6 | 2.4 | 0.3 | 2.6 | 0.4 | 16.1 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

program group members (and 6.9 percent of program group enrollees). The small differences persisted throughout the first three years of follow-up period but are not statistically significant. The difference between the average number of weeks in treatment was very small (1.34 weeks for the control group and 1.31 weeks for the program group). There were few differences in the places where treatment was received among those treated.

### 5. Impacts on Health

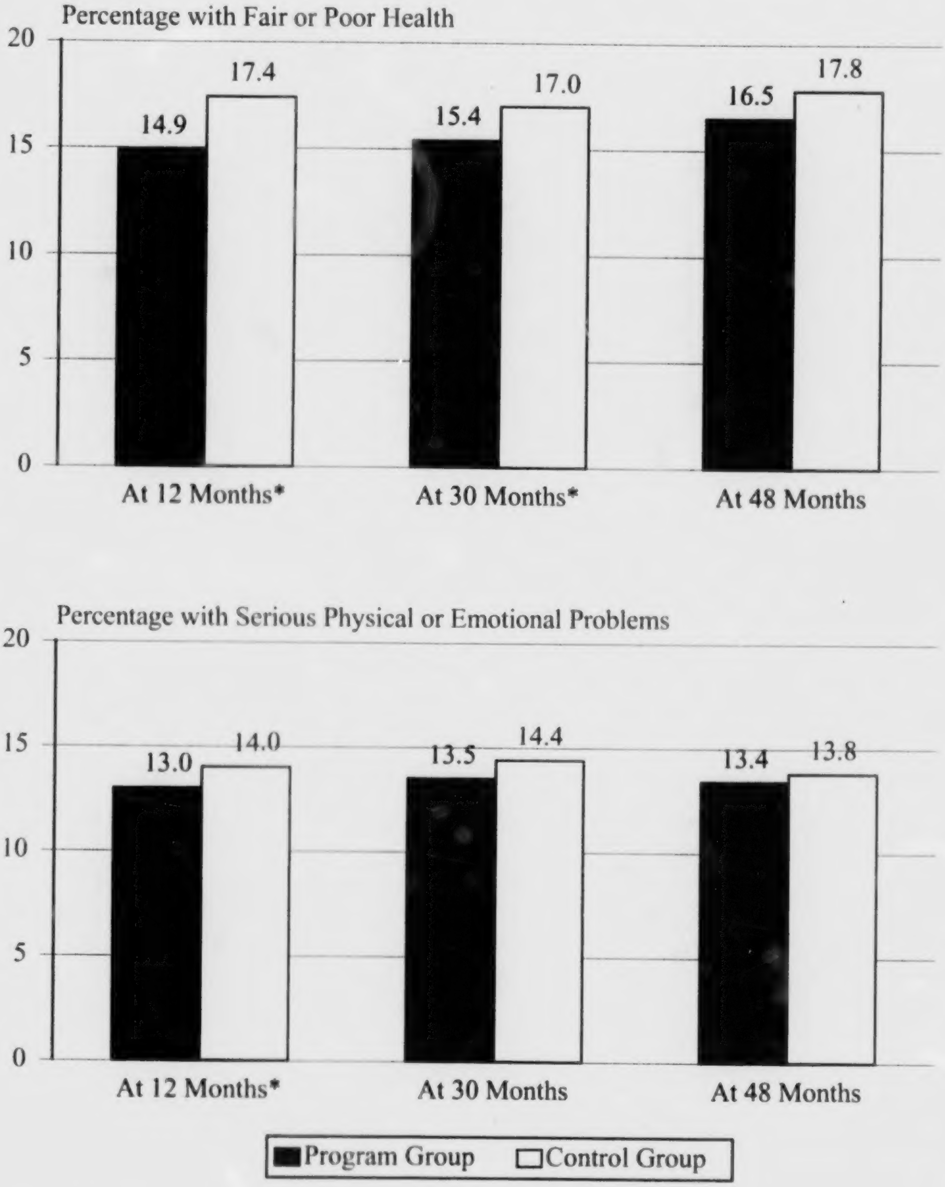
Job Corps significantly improved participants' self-reported health status at the 12-, 30-, and 48-month interview dates (Figure VII.13 and Table VII.15). About 17.4 percent of control group members reported that they were in fair or poor health at 12 months, compared to about 15 percent of program group members. This 2.5 percentage point impact per eligible applicant translates to a 3.5 percentage point impact per participant--or a 19 percent reduction in fair or poor health due to program participation. The impacts were slightly smaller at 30 and 48 months but are still statistically significant at the 10 percent level. We find a similar pattern on the prevalence of those who reported serious physical or emotional problems. Thus, it appears that health services and health education provided by Job Corps contributed to modest improvements in participants' perceived health status during both the in-program and postprogram periods.

### 6. Impacts on Mortality

When locating sample members for interviews, we tracked deaths and confirmed each reported one. The impact on deaths is the sum of the impact on health-related and accident-related deaths and the impact on murder and other crime-related deaths. Our ability to measure such impacts precisely and attribute them to one of the specific causes, however, is limited by the rarity of death and the difficulty of accurately identifying and classifying the circumstances of each death.

FIGURE VII.13

HEALTH STATUS AT THE 12-, 30-, AND 48-MONTH INTERVIEWS



Source: 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VII.15

## IMPACTS ON HEALTH STATUS

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Health Status at 12 Months (Percentages) | | | | | | |
| Excellent | 40.7 | 37.7 | 3.0***[d] | 41.0 | 4.2***[d] | 11.3 |
| Good | 44.4 | 44.9 | -0.5 | 44.2 | -0.7 | -1.5 |
| Fair | 13.4 | 15.6 | -2.1 | 13.4 | -2.9 | -18.0 |
| Poor | 1.5 | 1.9 | -0.4 | 1.5 | -0.6 | -26.7 |
| Fair or Poor | 14.9 | 17.4 | -2.5*** | 14.9 | -3.5*** | -19.0 |
| Health Status at 30 Months (Percentages) | | | | | | |
| Excellent | 39.4 | 36.8 | 2.6**[d] | 40.2 | 3.6**[d] | 10.0 |
| Good | 45.2 | 46.2 | -1.1 | 44.9 | -1.5 | -3.2 |
| Fair | 13.8 | 15.2 | -1.3 | 13.3 | -1.9 | -12.2 |
| Poor | 1.6 | 1.8 | -0.2 | 1.6 | -0.3 | -16.7 |
| Fair or Poor | 15.4 | 17.0 | -1.6** | 14.9 | -2.2** | -12.7 |
| Health Status at 48 Months (Percentages) | | | | | | |
| Excellent | 38.8 | 37.2 | 1.6 | 39.2 | 2.3 | 6.1 |
| Good | 44.7 | 45.0 | -0.3 | 44.2 | -0.4 | -1.0 |
| Fair | 14.9 | 16.0 | -1.2 | 15.0 | -1.6 | -9.6 |
| Poor | 1.6 | 1.8 | -0.2 | 1.6 | -0.2 | -12.8 |
| Fair or Poor | 16.5 | 17.8 | -1.3* | 16.6 | -1.8* | -9.9 |
| Percentage with Serious Physical or Emotional Problems That Limited the Amount of Work That Could Be Done or Other Regular Daily Activities | | | | | | |
| At 12 months | 13.0 | 14.0 | -1.1 | 12.6 | -1.5 | -10.4 |
| At 30 months | 13.5 | 14.4 | -0.9 | 13.1 | -1.3 | -9.1 |
| At 48 months | 13.4 | 13.8 | -0.4 | 12.9 | -0.6 | -4.4 |
| Type of Serious Health Problem at 48 Months (Percentages)[e] | | | | | | |
| Physical injuries | 18.9 | 17.4 | 1.6 | 18.3 | 2.2 | 13.6 |
| Psychological problems | 21.7 | 22.2 | -0.5 | 21.9 | -0.7 | -3.0 |
| Muscle and extremity problems | 23.8 | 24.3 | -0.4 | 23.2 | -0.6 | -2.6 |
| Respiratory problems | 6.0 | 8.1 | -2.1 | 6.1 | -2.9 | -32.3 |
| Reproductive problems | 10.8 | 9.1 | 1.7 | 11.1 | 2.4 | 27.4 |
| Organ problems | 9.7 | 11.1 | -1.4 | 10.2 | -2.0 | -16.1 |
| Miscellaneous problems | 9.0 | 7.8 | 1.2 | 9.1 | 1.6 | 21.6 |
| Average Number of Weeks Since Random Assignment Had Serious Health Problem at 48 Months[e] | 43.6 | 44.2 | -0.6 | 42.6 | -0.8 | -1.9 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

TABLE VII.15 *(continued)*

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NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

[e] Figures pertain to those with a serious physical or emotional problem at 48 months.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

Job Corps reduced mortality, but the effect is not statistically significant. There were a total of 130 confirmed deaths (54 for the control group and 76 for the program group) during the 48 month study period. Mortality rate estimates, however, are difficult to pinpoint because they vary depending on what is assumed about the mortality rate for those who we were unable to locate for follow-up interviews. Nonetheless, under a range of alternative assumptions, we estimate that Job Corps reduced the probability of death by about 50 to 150 deaths per 100,000 participants. For example, the estimated impact was about -70 deaths per 100,000 youth assuming that the mortality rate was similar for those who we located and for those who we did not. As another example, the estimated impact was about -110 deaths per 100,000 youth assuming that we located all those who actually died.[16] These small program impacts, however, are not statistically significant.

We did not confirm the cause of death for each instance, although anecdotal evidence from field staff suggests that causes were evenly distributed among crime, health, and accidents.[17]

### 7. Impacts for Subgroups

The pattern of self-reported rates of alcohol and drug use across subgroups closely follows the pattern of criminal justice system involvement across subgroups (Tables H.3 to H.5). The percentage of control group members who reported using drugs was higher for those 16 and 17 than for the older groups at each interview point.[18] Similarly, among the control group, males had higher reported rates of drug use than females (11.7 percent, as compared to 6.3 percent). Residential designees had somewhat higher rates than nonresidential designees, and rates were higher for those without a high school credential at baseline than their counterparts. In addition, those with previous arrests were nearly twice as likely to report using drugs than those without arrests. Self-reports of drug use were similar by race and for those who applied before and after the ZT policies took effect. Self-reports of drug use did not decrease appreciably over time.

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[16] The death rate calculations were conducted using only those who lived in areas selected for in-person interviewing at baseline, because those in other areas who did not complete the baseline interview were not eligible for follow-up interviews (see Chapter III). The sample also included only those in the in-person areas who completed either a baseline or 12-month interview, because youths in these areas who did not complete one of these interviews were not eligible for 30- or 48-month interviews.

[17] The causes of death, which themselves were unconfirmed, included the following health-related reasons: cancer, drug overdose, heart attack, brain tumor, childbirth, and suicide; and the following accident-related reasons: motor vehicle crash, train crash, fire, rock-climbing, and drowning.

Program group members were less likely than control group members to report having used alcohol at 12 months for most subgroups. For nearly all subgroups, impacts on alcohol consumption at 30 and 48 months were not statistically significant.

We find no consistent Job Corps impacts on the use of illegal drugs for any subgroup at either 12, 30, or 48 months. Very few of the impacts are negative, and even fewer are statistically significant. Thus, it appears that Job Corps had little effect on reducing self-reported drug use for broad groups of students.

Only a minority of control group members in each subgroup (ranging from about 12 to 20 percent) reported being in fair or poor health at each interview. Job Corps had beneficial effects on health for most subgroups, although impacts were most pronounced for the oldest youths, for males, and for whites.

## E. FAMILY FORMATION AND CHILD CARE

For most young people, forming intimate, long-term relationships with other adults, having children, and providing for the physical and emotional needs of those children are important aspects of the transition to adulthood. In general, adults hope that young people will defer having children until they have completed their education, can provide for the physical and emotional needs of their children, and have the emotional maturity to cope with work and family life. Adults also hope young people will marry before they have children. Indeed, being a child in a single-parent family is one of the strongest predictors of child poverty. Accordingly, we examined the extent to which participation in Job Corps led youths to defer having children, to marry, and to take an active role in caring for the children that they have.

---

[18] Alcohol use, however, increased with age at 12 and 30 months, but not at 48 months.

We anticipate that Job Corps participation could have affected family formation decisions through several pathways. First, instilling responsibility is a major goal of the program's highly structured, intensive format. Second, the curriculum includes components that address parenting and family life directly. Third, new options and opportunities, which result from additional education and training and better employment prospects, may exert indirect effects on participants' decisions to form relationships, have children, and take care of their children.

A related set of outcomes pertain to the use of child care. About 30 percent of females and 11 percent of males in our sample had children at baseline (although only about 20 percent of fathers lived with all their children). Most of these children were very young (about 85 percent were younger than three years old). Furthermore, many had children during the follow-up period. Thus, many parents needed to find child care while they worked or participated in education and training programs.

We expect that the program group was more likely than the control group to use child care during the in-program period. Impacts on working or being in school were large during this period (see Chapter VI). In addition, most Job Corps students live at centers, and thus many parents in the program group had to find a place for their children to live for a substantial period of time while they participated in the program. In fact, an eligibility requirement for Job Corps is that program applicants with children must demonstrate that they have an adequate child care plan for the proposed period of enrollment. Consequently, it is likely that the program group had a larger demand for child care during the early part of the follow-up period.

It is more difficult to anticipate the effects of Job Corps participation on the use of child care after participants leave the program. On the one hand, Job Corps may decrease the use of child care in the postprogram period if Job Corps reduces the likelihood of having children. On the other hand, Job Corps may increase the demand for child care in the postprogram period, because Job Corps increases the employment and earnings of former participants. Which of these opposing effects is stronger is an empirical question.

This section presents impact findings on four groups of outcomes:

1. ***Fertility,*** including the likelihood of (1) bearing or fathering children during the 48 months after random assignment; (2) having children out of wedlock; and (3) for females, being pregnant at the time of the 48-month interview.

2. ***Custodial responsibility and parental support,*** including the percentage of parents who lived with all their children at the 48-month interview and, for males, the amount of time spent with their noncustodial children and the types of support provided.

3. ***Living arrangements and marital status,*** including the composition of the sample member's household at the 48-month interview; household size; and whether the sample member was married, living with a partner, never married, separated, divorced, or widowed at that time.

4. ***Child care utilization,*** including the likelihood and number of hours that the sample member used child care by year after random assignment and by type of arrangement.

All these measures were constructed using information collected in the follow-up interviews.

In contrast to other sections of this report, we present findings for males, females without children at random assignment, and females with children at random assignment, along with the overall findings. Substantial differences in roles and responsibilities across these gender groups lead us to take this approach. The section concludes with a brief discussion of impact findings for other subgroups.

As we will discuss, we find small impacts on family formation. Equal percentages of program and control group members had children during the 48-month follow-up period. Job Corps participation, however, did have a small effect on promoting independent living at the 48-month interview point. A slightly smaller percentage of program group members were living with their parents, and a slightly larger percentage were living with a partner and reported being the head of the household. Job Corps participation also led to increases in the use of child care during the first and fourth years after random assignment for females, but not for males.

### 1. Impacts on Fertility

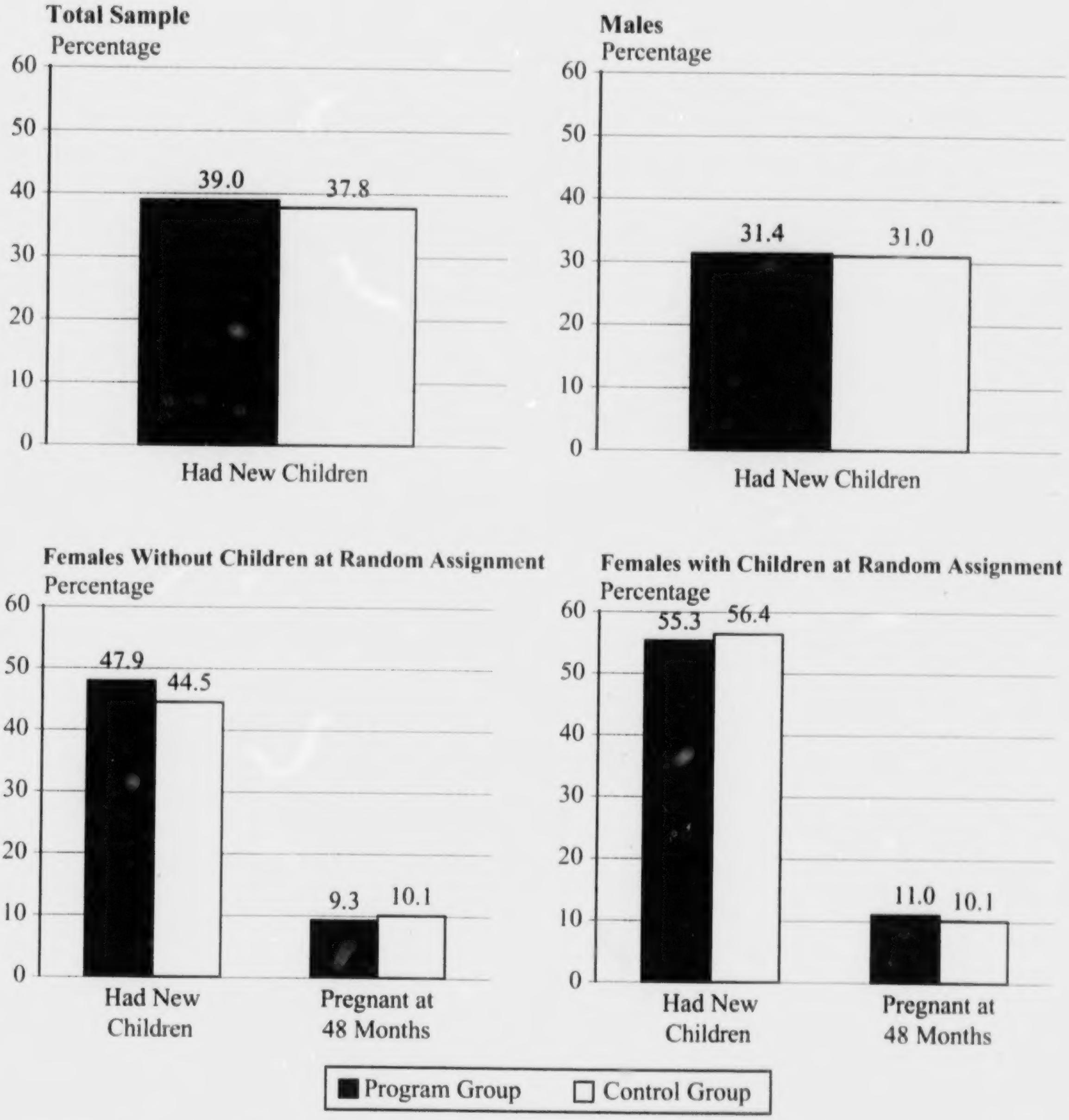
Job Corps had little or no effect on births during the 48 months after random assignment for the full sample and for the three gender subgroups (Figure VII.14 and Table VII.16). The birth rate was about 38 percent for all program and control group members: about 31 percent for males, 45 to 48 percent for females without children at random assignment, and 56 percent for females with children at random assignment. About 75 percent of those with new children had only one child. More than 80 percent of births were out of wedlock for each gender group. About 10 percent of females in the control and program groups were pregnant at the 48-month interview. None of the small differences between the program and control groups are statistically significant at the 5 percent level.

### 2. Impacts on Custodial Responsibility

An important dimension of parental responsibility is providing support to one's children. To assess the extent to which Job Corps influenced this support, we estimated impacts on the percentage

FIGURE VII.14

FERTILITY DURING THE 48 MONTHS AFTER RANDOM ASSIGNMENT FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN



Source: Baseline and 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

## TABLE VII.16

## IMPACTS ON FERTILITY FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Total Sample** | | | | | | |
| Percentage Had Children During the 48 Months After Random Assignment | 39.0 | 37.8 | 1.2 | 37.2 | 1.7 | 4.8 |
| Number of Children | | | | | | |
| 0 | 70.9 | 72.2 | -1.3 | 72.1 | -1.9 | -2.5 |
| 1 | 22.4 | 21.0 | 1.5 | 21.6 | 2.0 | 10.4 |
| 2 or more | 6.7 | 6.8 | -0.1 | 6.3 | -0.2 | -2.9 |
| (Average) | 0.5 | 0.5 | 0.0 | 0.5 | 0.0 | 3.3 |
| Percentage Had Children Out of Wedlock | 32.5 | 32.0 | 0.5 | 30.9 | 0.7 | 2.4 |
| Percentage of Females Pregnant at the 48-Month Interview | 9.8 | 10.0 | -0.2 | 9.9 | -0.3 | -2.7 |
| **Males** | | | | | | |
| Percentage Had Children During the 48 Months After Random Assignment | 31.4 | 31.0 | 0.3 | 29.5 | 0.4 | 1.5 |
| Number of Children | | | | | | |
| 0 | 84.0 | 84.2 | -0.2 | 84.6 | -0.3 | -0.3 |
| 1 | 13.1 | 12.4 | 0.7 | 12.7 | 1.0 | 8.2 |
| 2 or more | 2.9 | 3.4 | -0.5 | 2.8 | -0.7 | -20.1 |
| (Average) | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 0.0 |
| Percentage Had Children Out of Wedlock | 25.9 | 25.8 | 0.1 | 24.2 | 0.1 | 0.3 |
| **Females Without Children at Random Assignment** | | | | | | |
| Percentage Had Children During the 48 Months After Random Assignment | 47.9 | 44.5 | 3.5* | 47.6 | 4.9* | 11.4 |
| Number of Children | | | | | | |
| 0 | 53.8 | 57.2 | -3.3 | 54.0 | -4.7 | -8.0 |
| 1 | 34.7 | 31.0 | 3.8 | 35.1 | 5.3 | 17.9 |
| 2 or more | 11.4 | 11.8 | -0.4 | 11.0 | -0.6 | -5.2 |
| (Average) | 0.6 | 0.6 | 0.0 | 0.6 | 0.0 | 6.7 |
| Percentage Had Children Out of Wedlock | 40.7 | 39.4 | 1.2 | 40.5 | 1.7 | 4.4 |
| Percentage Pregnant at the 48-Month Interview | 9.3 | 10.1 | -0.8 | 9.5 | -1.1 | -10.4 |

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TABLE VII.16 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Females with Children at Random Assignment** | | | | | | |
| Percentage Had Children During the 48 Months After Random Assignment | 55.3 | 56.4 | -1.1 | 54.4 | -1.8 | -3.2 |
| Number of Children | | | | | | |
| 0 | 47.8 | 47.7 | 0.2 | 48.3 | 0.3 | 0.6 |
| 1 | 38.4 | 40.6 | -2.2 | 37.4 | -3.5 | -8.5 |
| 2 or more | 13.8 | 11.8 | 2.0 | 14.3 | 3.2 | 29.1 |
| (Average) | 0.7 | 0.7 | 0.0 | 0.7 | 0.0 | 0.9 |
| Percentage Had Children Out of Wedlock | 45.4 | 45.5 | 0.0 | 44.1 | 0.0 | -0.1 |
| Percentage Pregnant at the 48-Month Interview | 11.0 | 10.1 | 1.0 | 11.3 | 1.6 | 16.3 |
| **Total Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

of parents who lived with their children, and the types of support that were provided by males who did not live with their children (Figure VII.15 and Table VII.17).

We find large gender differences in the percentage of parents who lived with their children, but no impacts on this custodial measure. Overall, about 47 percent of youths in both research groups had children (including children born before and after random assignment and children who lived with the sample member and those who did not). Only about 42 percent of male parents in both groups lived with all their children. In contrast, nearly all females lived with their children.

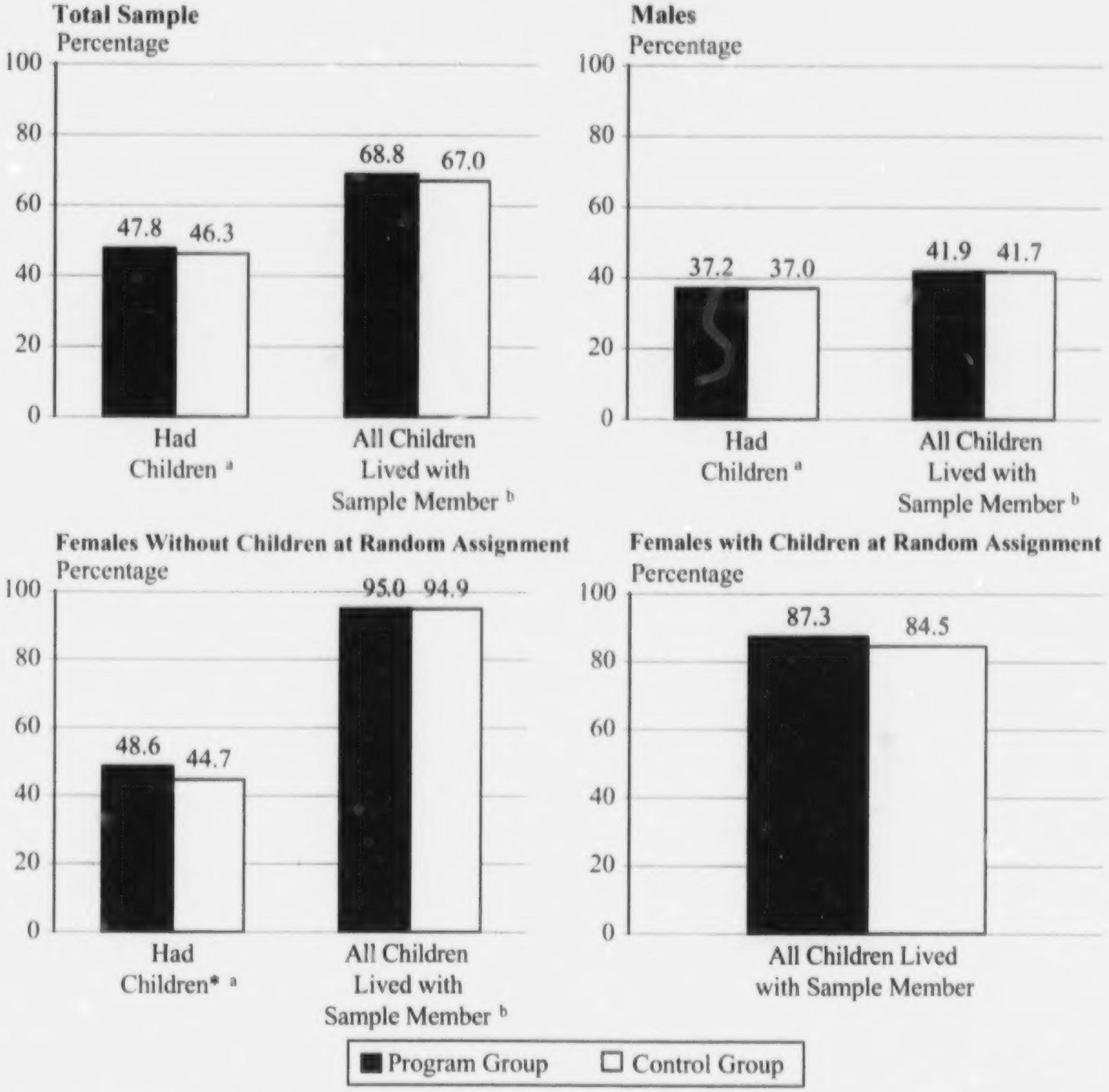
Because nearly all females lived with their children, we examined impacts on measures of custodial responsibility only for males. There were, however, no program impacts on these custodial responsibility measures. Among male parents who did not live with all their children, we find that most did not spend a substantial amount of time with their absent children, but most reported that they provided some support. Less than half in each research group said they had often spent time with their absent children in the prior three months. About a quarter reported that they never spent time with them. More than 80 percent, however, reported that they provided some type of support; about three-fourths provided money (about 55 percent on a regular basis), and the percentages who provided food, child care items, household items, clothing, toys, medicine, and babysitting ranged from about 45 to 70 percent.

### 3. Impacts on Living Arrangements and Marriage

The living arrangements of control group members at the 48-month interview differed across the gender groups (Table VII.18). In total, about 35 percent of control group members were living with their parents. Not surprisingly, this figure was lower than the 65 percent figure at baseline (Schochet 1998a) and the 43 percent figure at 30 months (Schochet et al. 2000), because some

## FIGURE VII.15

THE PRESENCE OF CHILDREN AND CUSTODIAL RESPONSIBILITY AT 48 MONTHS FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT



**Total Sample**

**Males**

**Females Without Children at Random Assignment**

**Females with Children at Random Assignment**

Source: Baseline and 12-, 30-, and 48-month follow-up interviews for those who completed 48-month interviews.

*Difference between the mean outcome for program and control group members is statistically significant at the 5 percent level. This difference is the estimated impact per eligible applicant.

[a] Includes children born before and after random assignment.

[b] Estimates pertain to parents only.

## TABLE VII.17

## IMPACTS ON CUSTODIAL RESPONSIBILITY AT 48 MONTHS FOR MALES

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage Had Children at the 48-Month Interview[d] | 37.2 | 37.0 | 0.2 | 34.9 | 0.2 | 0.7 |
| Percentage of Sample Members Who Lived with: | | | | | | |
| All their children[e] | 41.9 | 41.7 | 0.2 | 42.2 | 0.2 | 0.6 |
| Some of their children[e] | 5.9 | 6.0 | 0.0 | 6.3 | -0.1 | -0.9 |
| Percentage of Absent Children Who Lived with Their Other Parent[f] | 91.1 | 93.8 | -2.7* | 91.4 | -3.6* | -3.8 |
| Time Spent with Children in the Past Three Months (Percentages)[f] | | | | | | |
| Often | 44.3 | 43.4 | 0.9 | 43.1 | 1.2 | 2.8 |
| Sometimes | 18.4 | 21.2 | -2.8 | 17.9 | -3.8 | -17.5 |
| Rarely | 9.1 | 11.8 | -2.6 | 9.7 | -3.5 | -26.8 |
| Never | 28.2 | 23.6 | 4.6 | 29.3 | 6.1 | 26.6 |
| Percentage Currently Provided Type of Support[f] | | | | | | |
| Any | 81.8 | 82.9 | -1.0 | 83.1 | -1.4 | -1.6 |
| Food | 62.5 | 61.0 | 1.5 | 62.9 | 2.0 | 3.3 |
| Child care items | 62.2 | 61.5 | 0.7 | 62.3 | 0.9 | 1.5 |
| Household items | 53.8 | 51.6 | 2.2 | 54.0 | 2.9 | 5.8 |
| Clothing | 71.9 | 72.0 | 0.0 | 72.3 | -0.1 | -0.1 |
| Toys | 70.5 | 70.5 | 0.0 | 71.6 | 0.0 | 0.0 |
| Medicine | 59.0 | 56.6 | 2.4 | 58.8 | 3.2 | 5.7 |
| Babysitting | 45.4 | 47.1 | -1.7 | 45.1 | -2.2 | -4.7 |
| Money | 74.6 | 75.1 | -0.5 | 76.0 | -0.6 | -0.8 |
| Other | 16.7 | 15.2 | 1.5 | 16.3 | 2.1 | 14.6 |
| Percentage Gave Money[f] | | | | | | |
| In the past month | 65.3 | 64.7 | 0.6 | 66.3 | 0.8 | 1.2 |
| Occasionally | 19.3 | 18.1 | 1.2 | 20.7 | 1.7 | 8.8 |
| On a regular basis | 55.2 | 56.9 | -1.7 | 55.3 | -2.3 | -3.9 |
| Average Amount of Money Gave in the Past Month (in Dollars)[f] | 153.9 | 169.9 | -16.0 | 158.6 | -21.5 | -12.0 |
| **Sample Size** | **3,741** | **2,787** | **6,528** | **2,833** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

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TABLE VII.17 *(continued)*

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[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] Includes children born before and after random assignment.

[e] Estimates pertain to parents only.

[f] Estimates pertain to parents who did not live with all their children.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE VII.18

### IMPACTS ON LIVING ARRANGEMENTS AT THE 48-MONTH INTERVIEW FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Total Sample** | | | | | | |
| Household Membership | | | | | | |
| Living with either parent | 31.8 | 34.7 | -2.9**[d] | 32.4 | -4.0**[d] | -11.1 |
| Living with another adult relative | 24.3 | 22.2 | 2.0 | 23.5 | 2.8 | 13.5 |
| Living with adult nonrelative | 18.0 | 17.3 | 0.7 | 18.8 | 1.0 | 5.6 |
| Living with no other adults | 19.9 | 19.4 | 0.5 | 19.7 | 0.6 | 3.4 |
| In Job Corps, incarcerated, institutionalized, or homeless | 6.0 | 6.3 | -0.3 | 5.6 | -0.4 | -6.6 |
| Sample Member Is Head of Household | 51.8 | 50.2 | 1.6 | 51.2 | 2.2 | 4.4 |
| Number in Household | | | | | | |
| 1 | 9.6 | 10.0 | -0.3 | 10.5 | -0.4 | -4.0 |
| 2 | 20.8 | 20.2 | 0.7 | 20.9 | 0.9 | 4.6 |
| 3 | 25.4 | 24.6 | 0.8 | 25.5 | 1.1 | 4.6 |
| 4 | 19.7 | 19.5 | 0.2 | 19.5 | 0.3 | 1.7 |
| 5 or more | 24.4 | 25.8 | -1.4 | 23.6 | -1.9 | -7.5 |
| (Average) | 3.5 | 3.6 | 0.0 | 3.5 | -0.1 | -1.9 |
| **Males** | | | | | | |
| Household Membership | | | | | | |
| Living with either parent | 34.6 | 37.6 | -3.0* | 35.2 | -4.1* | -10.4 |
| Living with another adult relative | 24.5 | 22.1 | 2.4 | 23.6 | 3.3 | 16.1 |
| Living with adult nonrelative | 19.4 | 18.4 | 1.0 | 20.0 | 1.4 | 7.3 |
| Living with no other adults | 12.1 | 12.1 | 0.0 | 12.7 | 0.0 | 0.1 |
| In Job Corps, incarcerated, institutionalized, or homeless | 9.4 | 9.8 | -0.4 | 8.5 | -0.6 | -6.3 |
| Sample Member Is Head of Household | 49.4 | 49.0 | 0.4 | 49.5 | 0.6 | 1.2 |
| Number in Household | | | | | | |
| 1 | 11.9 | 11.6 | 0.3 | 12.5 | 0.3 | 2.8 |
| 2 | 19.8 | 20.3 | -0.5 | 19.8 | -0.7 | -3.5 |
| 3 | 25.5 | 24.6 | 0.9 | 25.5 | 1.3 | 5.3 |
| 4 | 20.0 | 18.4 | 1.6 | 20.0 | 2.2 | 12.4 |
| 5 or more | 22.8 | 25.1 | -2.3 | 22.2 | -3.1 | -12.2 |
| (Average) | 3.4 | 3.5 | -0.1 | 3.4 | -0.1 | -2.5 |
| **Females Without Children at Random Assignment** | | | | | | |
| Household Membership | | | | | | |
| Living with either parent | 31.3 | 34.7 | -3.4 | 30.7 | -4.8 | -13.6 |
| Living with another adult relative | 24.9 | 22.8 | 2.1 | 24.0 | 3.0 | 14.1 |
| Living with adult nonrelative | 17.9 | 17.4 | 0.5 | 18.8 | 0.7 | 4.1 |
| Living with no other adults | 24.9 | 23.9 | 1.0 | 25.5 | 1.4 | 5.9 |

TABLE VII.18 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| In Job Corps, incarcerated, institutionalized, or homeless | 1.0 | 1.2.0 | -0.2 | 1.0 | -0.3 | -21.8 |
| Sample Member Is Head of Household | 47.7 | 45.0 | 2.7 | 47.1 | 3.8 | 8.7 |
| Number in Household | | | | | | |
| 1 | 8.6 | 10.1 | -1.4 | 9.2 | -2.0 | -18.0 |
| 2 | 25.9 | 23.0 | 2.9 | 25.5 | 4.1 | 19.3 |
| 3 | 24.7 | 24.8 | -0.1 | 25.6 | -0.1 | -0.5 |
| 4 | 17.0 | 18.2 | -1.2 | 17.0 | -1.7 | -8.9 |
| 5 or more | 23.8 | 24.0 | -0.2 | 22.6 | -0.3 | -1.3 |
| (Average) | 3.5 | 3.5 | 0.0 | 3.4 | 0.0 | -0.1 |
| **Females with Children at Random Assignment** | | | | | | |
| Household Membership | | | | | | |
| Living with either parent | 19.0 | 20.5 | -1.5 | 20.1 | -2.5 | -11.1 |
| Living with another adult relative | 21.5 | 21.6 | -0.1 | 21.8 | -0.1 | -0.4 |
| Living with adult nonrelative | 11.9 | 11.4 | 0.5 | 11.5 | 0.8 | 7.4 |
| Living with no other adults | 46.3 | 45.5 | 0.8 | 45.6 | 1.2 | 2.8 |
| In Job Corps, incarcerated, institutionalized, or homeless | 1.3 | 1.0 | 0.3 | 1.0 | 0.6 | 115.2 |
| Sample Member Is Head of Household | 73.6 | 69.7 | 4.0* | 73.1 | 6.4* | 9.6 |
| Number in Household | | | | | | |
| 1 | 2.2 | 2.0 | 0.2 | 3.0 | 0.3 | 11.1 |
| 2 | 13.2 | 11.6 | 1.6 | 13.9 | 2.6 | 22.5 |
| 3 | 26.7 | 23.9 | 2.7 | 25.3 | 4.4 | 21.1 |
| 4 | 24.8 | 28.5 | -3.7 | 23.4 | -6.0 | -20.3 |
| 5 or more | 33.1 | 33.9 | -0.8 | 34.4 | -1.3 | -3.6 |
| (Average) | 4.0 | 4.2 | -0.1 | 4.0 | -0.2 | -5.1 |
| **Total Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

sample members moved away from home as they grew older. The percentage living with their parents was lower for females with children (21 percent) than for females without children (35 percent) and males (38 percent). Conversely, females with children were much more likely than the other gender groups to live with no other adults (46 percent, as compared to 24 percent for females without children and only 12 percent for males).[19] About 22 percent of each gender group lived with another adult relative, and the likelihood of living with adult nonrelatives was about 11 percent for females without children and about 18 percent for the other two gender groups. About 10 percent of male control group members were incarcerated, institutionalized, or homeless at the 48-month interview.

About one-half the control group reported being the head of the household at 48 months. This figure, however, was about 70 percent for females with children, who as discussed, were more likely than the other gender groups to live with no other adults.

We find that program group members were slightly *less* likely than control group members to live with their parents, and slightly *more* likely to live with other adult relatives, adult nonrelatives and no other adults (Table VII.18). These differences together are statistically significant at the 5 percent level for the full sample. About 32 percent of program group members were living with their parents, as compared to 35 percent of control group members. A higher percentage of program group members were living with adult relatives (24 percent, compared to 22 percent), with adult nonrelatives (18 percent, compared to 17 percent), and with no other adults (20 percent, compared to 19 percent). Furthermore, program group members were slightly more likely to report being the head of the household (52 percent, compared to 50 percent). This same pattern holds for *each* gender group.

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[19] In total, about 19 percent were living with no other adults, which is nearly quadruple the baseline figure (5 percent), and larger than the 14 percent figure at 30 months.

We also find that Job Corps participation led to small increases on the likelihood of living with a partner (either married or unmarried) at the 48-month interview, although the impacts are not statistically significant (Table VII.19). About 15 percent of the program group was married, compared to 14 percent of the control group. Similarly, a higher percentage of the program group was living with a partner unmarried (16 percent, compared to 15 percent for the control group). Taken together, these findings imply that the estimated impact per eligible applicant on the likelihood of living with a partner (either married or unmarried) was about 2 percentage points (31 percent program and 29 percent control)--or an 8 percent increase per participant. These small impacts were found across the gender groups, although they were somewhat larger for females than for males.

In sum, we find some evidence that Job Corps participation slightly promotes independent living for males and females with and without children. This finding is consistent with the employment and earnings gains that participants experience after they leave Job Corps, as well as the social skills and awareness training that participants receive in the program.

### 4. Impacts on Child Care Use

About 30 percent of females and 11 percent of males in our sample had young children when they applied to Job Corps (although only about 20 percent of fathers lived with all their children). Furthermore, as discussed earlier in this section, nearly half of program group and control group members had children by the end of the 48-month follow-up period. Because virtually all sample members worked or engaged in education or training at some point during the follow-up period, many parents needed to find suitable child care while they engaged in these activities.

TABLE VII.19

## IMPACTS ON MARITAL STATUS AT 48 MONTHS FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Total Sample** | | | | | | |
| Never Married, Not Living Together | 64.7 | 66.1 | -1.4 | 65.7 | -1.9 | -2.8 |
| Married | 14.9 | 13.9 | 1.0 | 14.1 | 1.3 | 10.5 |
| Living Together | 16.1 | 15.4 | 0.7 | 16.2 | 0.9 | 5.9 |
| Separated, Divorced, or Widowed | 4.3 | 4.6 | -0.3 | 4.0 | -0.4 | -8.1 |
| Married or Living Together | 31.0 | 29.4 | 1.6* | 30.2 | 2.2* | 8.0 |
| **Males** | | | | | | |
| Never Married, Not Living Together | 66.0 | 66.7 | -0.7 | 66.9 | -1.0 | -1.4 |
| Married | 13.5 | 13.7 | -0.2 | 12.7 | -0.2 | -1.6 |
| Living Together | 17.1 | 16.1 | 1.0 | 17.0 | 1.3 | 8.4 |
| Separated, Divorced, or Widowed | 3.4 | 3.5 | -0.1 | 3.4 | -0.1 | -3.5 |
| Married or Living Together | 30.6 | 29.8 | 0.8 | 29.7 | 1.1 | 3.9 |
| **Females Without Children at Random Assignment** | | | | | | |
| Never Married, Not Living Together | 64.1 | 66.8 | -2.8 | 64.9 | -3.9 | -5.6 |
| Married | 16.2 | 12.9 | 3.3 | 15.6 | 4.7 | 42.9 |
| Living Together | 15.7 | 16.1 | -0.4 | 15.9 | -0.6 | -3.4 |
| Separated, Divorced, or Widowed | 4.0 | 4.2 | -0.2 | 3.6 | -0.3 | -6.7 |
| Married or Living Together | 31.9 | 29.0 | 2.9* | 31.5 | 4.1* | 15.1 |
| **Females with Children at Random Assignment** | | | | | | |
| Never Married, Not Living Together | 60.5 | 61.7 | -1.2 | 61.2 | -1.9 | -3.0 |
| Married | 17.9 | 17.4 | 0.5 | 17.3 | 0.8 | 4.9 |
| Living Together | 12.2 | 10.1 | 2.1 | 12.4 | 3.4 | 36.9 |
| Separated, Divorced, or Widowed | 9.4 | 10.8 | -1.4 | 9.0 | -2.3 | -20.0 |
| Married or Living Together | 30.1 | 27.5 | 2.6 | 29.7 | 4.2 | 16.3 |
| **Total Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

TABLE VII.19 *(continued)*

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NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

Job Corps had no influence on fertility or custodial responsibility. However, we still anticipate impacts on child care use during the follow-up period, for several reasons. First, we expect that the program group had higher rates of utilization than the control group during the first part of the 48-month period, when Job Corps enrollment among the program group was at its peak. Impacts on working or being in school were large during this period. In addition, most Job Corps students live at centers, so many parents in the program group needed to find a place where their children could live for a substantial period of time while they participated in the program. Thus, the program group probably had a larger demand for child care during the in-program period. Second, because Job Corps participation led to employment gains during the postprogram period, we also anticipate that participants used more child care later in the follow-up period. Job Corps participants' earnings gains may have also affected the types of arrangements that they used, because they may have been better able to afford day care and other paid arrangements.[20]

In this section, we discuss impact findings on the use of child care for the full sample and for the three gender groups.[21] We discuss first the arrangements used by the control group, and then the differences in the arrangements used by the program and control groups. The analysis was conducted using information from the baseline and follow-up interviews on the main child care arrangements used by parents for their youngest child while the parents were at work or enrolled in an education or training program (including Job Corps). Respondents reported child care information for each activity spell and thus could have used multiple types of arrangements. Parents who did not participate in employment or education activities after having children were not asked about their child care arrangements. Appendix I contains additional tables.

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[20] Potential increases in the use of child care as a result of participation in Job Corps are treated as costs to society in the benefit-cost analysis, because child care uses resources that otherwise could be used elsewhere in the economy (McConnell et al. 2001).

[21] Although appealing, we did not use for the analysis the sample of only those who had children at the 48-month point, because this sample may produce biased impact estimates due to potential differences in the composition of program and control group members who had children.

### a. Impacts on the Rate of Child Care Utilization and Time Spent in Child Care

Many control group members used child care during the 48-month period (Table VII.20). About 42 percent of all control group members, and more than 90 percent of those who had children reported the use of child care while they were working or in an education or training program. On average, the control group used about 5 hours of child care per week, which translates into nearly 13 hours per week over the 48 months among those who used child care.

The rate of child care utilization for the control group increased over time as fertility and activity rates increased. About 15 percent reported using child care in the first year after random assignment, and the figure more than doubled, to 33 percent, in year 4. Similarly, the average number of hours of child care use substantially increased from 2.9 hours per week in year 1 to 7.9 hours per week in year 4.

Not surprisingly, among the control group, females with children at baseline used more child care services in each year than males and females without children, especially early in the follow-up period (Table VII.20). About 65 percent of females with children used child care in year 1, and the rate was about 76 percent in year 4. The rate for females without children was only 4 percent in year 1, but increased substantially, to 31 percent, in year 4, when many were mothers. More males than females without children reported using child care during the first half of the follow-up period, but the rates for the two groups were similar during the second half. The relatively high rates of child care utilization for males is surprising, because although 37 percent had children, only about 40 percent of male parents lived with all their children at the 48-month point. Consequently, only about

## TABLE VII.20

## IMPACTS ON CHILD CARE UTILIZATION FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Total Sample** | | | | | | |
| Percentage Used Child Care, by Year After Random Assignment | | | | | | |
| All years | 43.7 | 41.6 | 2.1** | 41.9 | 2.9** | 7.4 |
| 1 | 17.3 | 15.1 | 2.1*** | 16.4 | 3.0*** | 22.1 |
| 2 | 23.7 | 23.4 | 0.3 | 21.7 | 0.5 | 2.2 |
| 3 | 31.7 | 30.7 | 1.0 | 29.6 | 1.4 | 4.9 |
| 4 | 35.2 | 33.3 | 1.9** | 33.3 | 2.6** | 8.6 |
| Average Number of Hours per Week Used Child Care, by Year | | | | | | |
| All years | 5.9 | 5.3 | 0.5*** | 5.4 | 0.7*** | 15.4 |
| 1 | 3.9 | 2.9 | 1.0*** | 3.9 | 1.5*** | 58.8 |
| 2 | 4.7 | 4.6 | 0.1 | 4.2 | 0.1 | 1.8 |
| 3 | 6.7 | 6.3 | 0.4 | 6.2 | 0.5 | 9.7 |
| 4 | 8.6 | 7.9 | 0.7** | 8.0 | 1.0** | 13.5 |
| **Males** | | | | | | |
| Percentage Used Child Care, by Year | | | | | | |
| All years | 35.2 | 35.1 | 0.1 | 33.9 | 0.1 | 0.3 |
| 1 | 11.5 | 10.7 | 0.8 | 11.2 | 1.0 | 9.9 |
| 2 | 18.0 | 18.3 | -0.3 | 16.5 | -0.4 | -2.6 |
| 3 | 24.4 | 25.3 | -0.8 | 23.0 | -1.1 | -4.7 |
| 4 | 26.8 | 26.2 | 0.5 | 25.4 | 0.7 | 2.9 |
| Average Number of Hours per Week Used Child Care, by Year | | | | | | |
| All years | 4.6 | 4.6 | 0.0 | 4.2 | 0.0 | -0.3 |
| 1 | 2.8 | 2.4 | 0.4 | 2.7 | 0.5 | 21.5 |
| 2 | 3.7 | 3.8 | -0.1 | 3.3 | -0.2 | -4.4 |
| 3 | 5.4 | 5.5 | -0.1 | 5.0 | -0.1 | -2.4 |
| 4 | 6.9 | 6.8 | 0.0 | 6.5 | 0.0 | 0.6 |
| **Females Without Children** | | | | | | |
| Percentage Used Child Care, by Year | | | | | | |
| All years | 40.0 | 35.1 | 5.0*** | 39.3 | 7.0*** | 21.6 |
| 1 | 4.1 | 4.1 | 0.0 | 2.8 | 0.0 | 0.6 |
| 2 | 14.5 | 14.2 | 0.3 | 13.4 | 0.4 | 3.3 |
| 3 | 27.8 | 24.1 | 3.7** | 27.2 | 5.3** | 24.0 |
| 4 | 35.5 | 30.8 | 4.7*** | 34.8 | 6.6*** | 23.6 |
| Average Number of Hours per Week Used Child Care, by Year | | | | | | |
| All years | 3.7 | 3.2 | 0.4* | 3.4 | 0.6* | 22.5 |
| 1 | 0.5 | 0.5 | 0.0 | 0.4 | 0.0 | -2.3 |
| 2 | 2.1 | 2.3 | -0.2 | 1.9 | -0.3 | -13.6 |
| 3 | 4.7 | 4.1 | 0.6* | 4.5 | 0.9* | 25.1 |
| 4 | 7.5 | 6.2 | 1.2** | 7.1 | 1.7** | 32.5 |

TABLE VII.20 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Females With Children** | | | | | | |
| Percentage Used Child Care, by Year | | | | | | |
| All years | 94.2 | 90.7 | 3.5*** | 95.9 | 5.7*** | 6.3 |
| 1 | 77.7 | 65.0 | 12.7*** | 85.3 | 20.5*** | 31.7 |
| 2 | 74.1 | 72.4 | 1.7 | 75.6 | 2.8 | 3.8 |
| 3 | 76.2 | 74.1 | 2.1 | 75.1 | 3.4 | 4.7 |
| 4 | 76.0 | 76.1 | 0.0 | 75.9 | -0.1 | -0.1 |
| Average Number of Hours per Week Used Child Care, by Year | | | | | | |
| All years | 18.2 | 15.0 | 3.2*** | 19.0 | 5.1*** | 36.8 |
| 1 | 18.1 | 11.1 | 7.0*** | 21.4 | 11.2*** | 109.9 |
| 2 | 16.2 | 15.0 | 1.2 | 16.3 | 1.9 | 13.4 |
| 3 | 18.3 | 16.4 | 1.8** | 18.0 | 3.0** | 19.7 |
| 4 | 19.7 | 17.6 | 2.1** | 20.0 | 3.4** | 20.8 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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15 percent of males had child care responsibilities. However, as discussed in the next section, many fathers reported that the child's mother was the "child care provider" for their noncustodial children.

Job Corps participation led to statistically significant increases in the use of child care for the full sample (Table VII.20). Over the 48-month period, a higher percentage of the program group than the control group used child care (44 percent, compared to 42 percent). Similarly, the average participant used an average of about 0.7 hours per week (146 hours in total) more than they would have if they had not enrolled in the program--an increase of about 15 percent per participant.

The positive estimated child care impacts over the 48-month period were due to positive estimated impacts in years 1 and 4. The estimated impact per participant on average hours of child care use was about 1.5 hours per week in year 1 (a period when many program group members were enrolled in Job Corps). The estimated child care impacts were small and not statistically significant in years 2 and 3. In year 4, however, the impact per participant on child care use was 1 hour per week and statistically significant. The year 4 findings are consistent with the employment gains that participants experienced during the latter part of the follow-up period.

Job Corps substantially increased the use of child care for females but not for males (Table VII.20). For females with children, the estimated impact on hours of child care use was very large in year 1 (about 11 hours per week for participants), because mothers in the program group who enrolled in Job Corps needed to use substantial amounts of child care while they were in the program. The estimated impacts on hours of child care use in years 3 and 4 were also statistically significant for these mothers. For females without children at baseline, Job Corps also led to increases in child care use in years 3 and 4, but not in years 1 and 2, because only a small percentage of these females had children then. Job Corps had no effect on child care utilization for males, because only about 15 percent of them were fathers and living with all their children at the 48-month point, so only a small percentage needed to find child care.

### b. Impacts on Child Care Utilization by Type of Arrangement

Not surprisingly, the most common child care arrangement for control group members was care by relatives (including the child's other parent, grandparents, or other relatives; Tables VII.21 and I.4). Overall, about 37 percent (and nearly 90 percent of those who used child care) used relative care at some point during the 48-month period.[22] About one-quarter of children were cared for by the child's other parent, 16 percent by grandparents, and 7.5 percent by other relatives. Nearly 11 percent of children (and one-quarter of those in child care) were cared for in day care centers, and 7 percent were cared for by nonrelatives (about three-quarters of whom were paid). Very few used care provided by their employer or school.

Over time, child care users became somewhat more likely to use nonrelative care and day care and less likely to use relative care as their incomes increased and their children became older (Table VII.21). Furthermore, a larger percentage of children were in kindergarten or elementary school in year 4 than in year 1.

The child care arrangements used by control group members differed markedly by gender (Tables I.1 to I.4). About 85 percent of males who used child care reported that their children were cared for by the child's mother. Thus, it appears that many fathers reported a child care arrangement *even if* they were not living with their children. This finding explains the discrepancy between the relatively high reported rates of child care use for males and the small percentage of fathers who

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[22] The 90 percent figure for the control group, for example, is calculated by dividing the percentage who used relative care (36.9 percent) by the percentage who used any child care (41.6 percent, as shown in Table VII.20).

## TABLE VII.21

## IMPACTS ON CHILD CARE UTILIZATION, BY TYPE OF ARRANGEMENT AND YEAR

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Percentage Ever Used Type of Arrangement During the 48 Months After Random Assignment** | | | | | | |
| Any Relative | | | | | | |
| Other parent | 38.3 | 36.9 | 1.4 | 36.6 | 1.9 | 5.6 |
| Grandparent | 24.8 | 24.0 | 0.8 | 24.6 | 1.1 | 4.8 |
| Other relative | 17.4 | 16.2 | 1.2* | 16.5 | 1.7* | 11.6 |
| Nonrelative | 7.9 | 7.5 | 0.3 | 6.8 | 0.4 | 6.9 |
| Paid | 6.1 | 5.6 | 0.4 | 5.9 | 0.6 | 11.6 |
| Unpaid | 2.4 | 2.2 | 0.2 | 2.3 | 0.3 | 16.4 |
| Day care center, nursery school, or preschool | 11.1 | 10.5 | 0.6 | 9.8 | 0.8 | 8.9 |
| Kindergarten or elementary school | 2.2 | 2.1 | 0.0 | 1.8 | 0.1 | 3.1 |
| On site at education program or job | 1.2 | [illegible] | 0.6*** | 1.2 | 0.8*** | 204.8 |
| **Percentage Ever Used Type of Arrangement in Year 1** | | | | | | |
| Any Relative | | | | | | |
| Other parent | 14.0 | 12.2 | 1.8*** | 13.9 | 2.5*** | 21.9 |
| Grandparent | 7.2 | 6.8 | 0.4 | 7.4 | 0.6 | 8.1 |
| Other relative | 6.5 | 4.5 | 2.1*** | 6.7 | 2.9*** | 74.9 |
| Nonrelative | 1.9 | 2.0 | -0.1 | 1.6 | -0.1 | -8.0 |
| Paid | 1.1 | 1.3 | -0.2 | 0.9 | -0.3 | -25.2 |
| Unpaid | 0.6 | 0.3 | 0.3** | 0.5 | 0.4** | 392.5 |
| Day care center, nursery school, or preschool | 3.0 | 2.8 | 0.2 | 2.6 | 0.3 | 10.8 |
| Kindergarten or elementary school | 0.2 | 0.3 | -0.1 | 0.1 | -0.1 | -55.4 |
| On site at education program or job | 0.5 | 0.2 | 0.3** | 0.5 | 0.4** | 467.8 |
| **Percentage Ever Used Type of Arrangement in Year 4** | | | | | | |
| Any Relative | | | | | | |
| Other parent | 26.6 | 25.4 | 1.2 | 25.0 | 1.7 | 7.1 |
| Grandparent | 15.3 | 14.8 | 0.5 | 15.1 | 0.8 | 5.3 |
| Other relative | 9.4 | 8.7 | 0.7 | 8.4 | 1.0 | 13.0 |
| Nonrelative | 4.1 | 3.9 | 0.2 | 3.4 | 0.2 | 7.2 |
| Paid | 3.5 | 2.9 | 0.6* | 3.5 | 0.8* | 1.8 |
| Unpaid | 0.9 | 1.0 | -0.1 | 0.9 | -0.1 | -7.2 |
| Day care center, nursery school, or preschool | 7.4 | 7.2 | 0.2 | 6.7 | 0.3 | 4.9 |
| Kindergarten or elementary school | 1.6 | 1.6 | 0.0 | 1.3 | 0.0 | -1.5 |
| On site at education program or job | 0.3 | 0.2 | 0.1 | 0.3 | 0.2 | 141.3 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

TABLE VII.21 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

lived with their children. Only a small percentage of males reported using other types of arrangements.

The most common child care arrangement for mothers was care by grandparents, although many also used other types of care. Among child care users, more than one-half had grandparents watch their children, and about 45 percent of those with children at baseline placed their children in day care centers. About one-quarter to one-third of child care users had the child's father, other relatives, or nonrelatives watch their children at some point during the four-year follow-up period. The share of all care that was day care and nonrelative care increased over time.

For the full sample, the program group was slightly more likely than the control group to use each type of child care arrangement over the follow-up period, although the estimated impacts were small (Tables VII.21 and I.4). Thus, the statistically significant positive impacts on child care use overall were the sum of small impacts on the use of various types of child care arrangements. Interestingly, the impacts on the use of grandparent care and care provided by employers or education programs were the only types of care that were statistically significant at the 5 percent level. These results are consistent with our findings in Chapter IV that most parents who enrolled in Job Corps used grandparent care while they attended the program, and that about 5 percent of program participants used child care provided by Job Corps.

Conditional on using child care, Job Corps had no effect on the types of arrangements that were used. In other words, similar percentages of child care users in the program and control groups used relative care, nonrelative care, and day care. Thus, there is no evidence that the earnings gains of program participants led child care users to pursue more costly types of care.

As with the overall use of child care, impacts on child care use by type of arrangement differed by gender (Tables I.1 to I.4). Job Corps had no effect on the use of any type of child care for males.

Job Corps participation, however, led to increases in the use of grandparent care, day care, and on-site care for females with children at baseline, and to increases in the use of relative and nonrelative care later in the follow-up period for females without children at baseline.

### 5. Impacts for Other Subgroups

Family formation outcomes among the control group differed somewhat across key subgroups defined by baseline characteristics (Table I.5). For example, the older youths were more likely to live with a partner than the younger ones, and were less likely to live with their parents. Surprisingly, however, the fertility rate did not increase with age. Among the racial and ethnic groups, whites were the most likely to have lived with a partner and the least likely to have had children, whereas we find the reverse for African Americans. The family formation measures were similar for residential and nonresidential designees within the gender groups.

Despite differences in the *levels* of the family formation outcomes across subgroups, the estimated *impacts* on these outcomes were similar across subgroups (Table I.5). The percentage of program and control group members who had new children and who lived with all their children were similar for most subgroups. Similarly, Job Corps slightly increased the likelihood of living with a partner and slightly decreased the likelihood of living with one's parents for nearly all subgroups. Tests of hypotheses that impacts were the same across subgroups were rarely rejected. Thus, during the 48 months after random assignment, it appears that for diverse groups of students, Job Corps participation had no effect on fertility and custodial responsibility, but had small effects on promoting independent living.

Finally, Job Corps led to increases in total hours of child care use for most subgroups (Table I.5). Importantly, the impacts on child care use were positive and statistically significant for both female residential and female nonresidential designees. This finding reflects the fact that nonresidential students with children must also find suitable child care for their children while enrolled in the program, and the fact that Job Corps participation led to increases in postprogram employment levels for both groups of females.

## F. MOBILITY

Youths served by Job Corps face many barriers to achieving self-sufficiency. Some of these barriers relate to family circumstances--for example, difficult or unstable living arrangements or lack of support from family members. Also, many youths live in neighborhoods where poverty rates are high and job opportunities scarce. A core element of the philosophy motivating Job Corps's residential component is that, for some, the home environment creates insurmountable barriers to succeeding in training and that removal from the home is necessary if the youth is to take full advantage of training. Indeed, living in a debilitating environment that precludes participation in other education and training programs is a key criterion for Job Corps eligibility.

This element of Job Corps raises the question of whether participation promotes mobility of students. Participation in Job Corps could affect the types of areas where students live after they leave the program because of job placement and location assistance, and because higher earnings could make some neighborhoods more affordable. However, many Job Corps students are believed to return to their home neighborhoods after leaving the program. Thus, we anticipate that impacts on mobility outcomes during the 48-month follow-up period are likely to be quite small.

We address two specific questions:

1. Do students return to the same areas that they lived in at the time of application?
2. Do students move to areas that offer opportunities different from those in the areas they came from?

To address these questions, we examined the following measures: (1) the distance in miles between the zip code of residence at application to Job Corps and the zip code at the time of the 48-month interview, (2) whether the sample member lived in the same state at application and at the 48-month interview, and (3) the characteristics of the counties of residence at application and at 48 months (using data from the 1998 Area Resource File [ARF]).[23] Most county measures in ARF that were used in the analysis were from the 1990 Census, so they pertain to the period before the 48-month interview date for all sample members (because the earliest interview was conducted in late 1998). Furthermore, the measures are broad because they are at the county level. However, the county measures provide an indication of the types of areas in which sample members lived.

We find that most control group members returned to the area they lived in before applying for Job Corps (Table VII.22). About half lived in the same zip code at 48 months as they did at application to Job Corps, and nearly three-quarters lived within 10 miles; the median distance was 0 miles (not shown). Only about 16 percent lived more than 50 miles away. Furthermore, about 88 percent lived within the same state. Surprisingly, measures of mobility were similar for males and females. In addition, measures of mobility at 48 months were very similar to those at 30 months (see Schochet et al. 2000).

Job Corps led to a small increase in mobility. Slightly fewer of the program group lived less than 10 miles from where they lived at application (72.8 percent, compared to 74.9 percent of the control group), and slightly more lived more than 50 miles away (17.0 percent, compared to 15.9 percent). Furthermore, the average distance was slightly farther for the program group (94 miles,

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[23] These data are made available by the Bureau of Health Professions at the Department of Health and Human Services.

TABLE VII.22

IMPACTS ON MOBILITY FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Total Sample** | | | | | | |
| Distance in Miles Between Zip Codes of Residence at Application to Job Corps and at the 48-Month Interview (Percentages) | | | | | | |
| 0 | 48.5 | 49.9 | -1.4 | 47.9 | -1.9 | -3.9 |
| 1 to 10 | 24.3 | 25.0 | -0.7 | 23.8 | -0.9 | -3.8 |
| 10 to 50 | 10.2 | 9.2 | 0.9 | 9.9 | 1.3 | 15.1 |
| 50 to 250 | 7.8 | 6.7 | 1.1 | 8.5 | 1.5 | 21.7 |
| 250 or more | 9.2 | 9.2 | 0.1 | 9.9 | 0.1 | 0.8 |
| (Average) | 93.7 | 85.5 | 8.1 | 100.3 | 11.3 | 12.7 |
| Lived in the Same State at Application to Job Corps and the 48-Month Interview | 87.8 | 88.4 | -0.6 | 86.9 | -0.8 | -0.9 |
| **Males** | | | | | | |
| Distance in Miles Between Zip Codes of Residence at Application to Job Corps and at the 48-Month Interview (Percentages) | | | | | | |
| 0 | 50.3 | 52.0 | -1.8 | 50.4 | -2.4 | -4.5 |
| 1 to 10 | 21.4 | 22.1 | -0.7 | 20.4 | -0.9 | -4.3 |
| 10 to 50 | 10.2 | 9.2 | 0.9 | 9.4 | 1.3 | 15.5 |
| 50 to 250 | 8.4 | 7.3 | 1.1 | 9.3 | 1.5 | 18.9 |
| 250 or more | 9.8 | 9.4 | 0.4 | 10.4 | 0.6 | 5.7 |
| (Average) | 104.9 | 87.1 | 17.8** | 111.5 | 23.9** | 27.2 |
| Lived in the Same State at Application to Job Corps and at the 48-Month Interview | 87.0 | 87.9 | -0.9 | 85.9 | -1.2 | -1.4 |
| **Females Without Children at Random Assignment** | | | | | | |
| Distance in Miles Between Zip Codes of Residence at Application to Job Corps and at the 48-Month Interview (Percentages) | | | | | | |
| 0 | 46.7 | 47.8 | -1.1 | 44.6 | -1.6 | -3.4 |
| 1 to 10 | 25.9 | 26.3 | -0.5 | 26.7 | -0.7 | -2.4 |
| 10 to 50 | 10.5 | 10.0 | 0.4 | 11.0 | 0.6 | 6.0 |
| 50 to 250 | 7.4 | 6.1 | 1.3 | 7.7 | 1.9 | 32.6 |
| 250 or more | 9.5 | 9.7 | -0.2 | 10.0 | -0.3 | -3.0 |
| (Average) | 84.6 | 93.3 | -8.7 | 86.6 | -12.2 | -12.4 |
| Lived in the Same State at Application to Job Corps and at the 48-Month Interview | 87.7 | 88.9 | -1.1 | 87.7 | -1.6 | -1.8 |

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TABLE VII.22 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Females with Children at Random Assignment** | | | | | | |
| Distance in Miles Between Zip Codes of Residence at Application to Job Corps and at the 48-Month Interview (Percentages) | | | | | | |
| 0 | 44.2 | 45.0 | -0.7 | 42.3 | -1.2 | -2.7 |
| 1 to 10 | 34.7 | 36.1 | -1.4 | 35.3 | -2.2 | -5.9 |
| 10 to 50 | 9.8 | 7.5 | 2.3 | 9.2 | 3.7 | 65.7 |
| 50 to 250 | 5.6 | 5.0 | 0.6 | 6.4 | 0.9 | 16.6 |
| 250 or more | 5.7 | 6.4 | -0.7 | 6.9 | -1.2 | -14.5 |
| (Average) | 59.7 | 57.1 | 2.5 | 71.0 | 4.1 | 6.1 |
| Lived in the Same State at Application to Job Corps and at the 48-Month Interview | 92.2 | 90.3 | 1.9 | 90.6 | 3.0 | 3.5 |
| **Total Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] The significance levels pertain to statistical tests for differences in the distribution of the outcome measure for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

compared to 86 miles for the control group) as was the median distance (1.3 miles, compared to 0 miles). In conjunction with the finding that members of the program group were slightly more likely to identify themselves as the head of household and slightly less likely to live with their parents, this finding on mobility suggests that participation in Job Corps had very modest effects on the likelihood a youth was living independently four years after application to Job Corps.

Table VII.23 displays selected characteristics of the county in which a typical sample member resided at program application and at 48 months. (Data for the 48-month point are shown by research status.) As a frame of reference, the table also shows county characteristics for the typical 20- to 24-year-old nationally.[24]

Several interesting results emerge from the table. First, and not surprisingly, Job Corps students usually come from areas more disadvantaged than the communities of typical youth nationally. Job Corps students, relative to the typical youth nationally, come from counties with higher poverty rates, lower median incomes, lower educational levels, higher unemployment rates, and lower housing values. Second, the characteristics of the counties that sample members lived in were similar at program application and at 48 months, which is consistent with our finding that many participants lived in the same areas at both points. Finally, we find no differences in the 48-month county characteristics for program and control group members (which is consistent with our finding of small impacts on mobility).

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[24] Our sample members were about 20 to 29 years old at the 48-month interview. However, the ARF does not contain population data for this age group, information that was needed to construct weights for calculating the national figures. Thus, we used the available 20- to 24-year figures instead.

## TABLE VII.23

## CHARACTERISTICS OF THE COUNTIES OF RESIDENCE AT APPLICATION TO JOB CORPS AND THE 48-MONTH INTERVIEW

| | | At the 48-Month Interview | | | |
|---|---|---|---|---|---|
| County Characteristic | At Application to Job Corps | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | National Population of Those 20 to 24 |
| Percentage of Persons with Incomes Below the Poverty Line in 1989 | 16.1 | 15.7 | 15.9 | -0.2 | 13.3 |
| Percentage of Families with Incomes Below the Poverty Line in 1989 | 12.7 | 12.3 | 12.5 | -0.2 | 10.1 |
| Median Family Income in 1989 (in Dollars) | 33,144 | 33,430 | 33,493 | -63 | 36,395 |
| Percentage of Households with Female Heads in 1990 | 19.4 | 19.1 | 19.3 | -0.2 | 17.1 |
| Percentage of Persons 25 or Older in 1990 Who Did Not Complete High School | 35.3 | 35.0 | 35.1 | -0.1 | 32.6 |
| Percentage of Persons 25 or Older in 1990 Who Completed Four Years of College | 19.3 | 19.4 | 19.4 | 0.0 | 21.0 |
| Percentage of the Population in Jail or in a Juvenile Home in 1990 | 0.5 | 0.5 | 0.5 | 0.0 | 0.5 |
| Percentage of the Population in Urban Areas in 1990 | 77.4 | 77.1 | 77.7 | -0.6 | 77.3 |
| Median Home Value in 1990 (in Dollars) | 86,855 | 85,110 | 87,991 | -2,881** | 103,497 |
| Unemployment Rate in 1996 | 6.2 | 6.0 | 6.1 | -0.1** | 5.5 |
| **Sample Size** | **11,313** | **6,828** | **4,485** | **11,313** | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews, and data from the 1998 Area Resource File.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# VIII. SUMMARY AND CONCLUDING OBSERVATIONS

This report has extensively documented the impacts of Job Corps on the employment and related outcomes of participants during the first four years after they were found eligible for Job Corps. In this chapter, we gather and summarize the main impact findings that suggest that Job Corps is an effective program for broad groups of students that it serves. In addition, we offer some concluding remarks that place these findings in a broader context.

## A. SUMMARY

The key findings on the impacts of Job Corps can be summarized as follows.

**Job Corps provided extensive education, training, and other services to the program group.** Follow-up interviews show that 73 percent of the program group enrolled in Job Corps and that 72 percent of enrollees (and just over half the full program group) participated in Job Corps for at least three months. The average period of participation per enrollee was eight months. Enrollees also participated extensively in the core Job Corps activities.

**Job Corps substantially increased the education and training services that program group participants received, and it improved their educational attainment.** Job Corps significantly increased the percentage of youth who attended an education or training program, as well as the amount and intensity of their education and training. It also focused more on vocational instruction than did the training available elsewhere. On average, Job Corps increased participants' time spent in education and training programs (both in and out of Job Corps) by about 1,000 hours, approximately the number in a regular 10-month school year. The impacts were equally large across all key subgroups of youths defined by their characteristics at baseline.

Job Corps substantially increased the receipt of certificates that it emphasizes: GED and vocational certificates. Among those without a high school credential at random assignment, about 42 percent of program group members (and 46 percent of program group participants) obtained a GED during the 48-month period, compared to only 27 percent of control group members (an impact of 15 percentage points per eligible applicant). Similarly, about 38 percent of program group members (and 45 percent of Job Corps participants) reported receiving a vocational certificate, compared to about 15 percent of control group members (an impact of 22 percentage points).

The program, however, had no effect on college attendance or completion.

**Job Corps generated positive employment and earnings impacts beginning in the third year after random assignment, and the impacts persisted through the end of the 48-month follow-up period.** In the last year of the 48-month follow-up period, participants earned about $22 per week (or $1,150 in total) more than they would have had they not enrolled in Job Corps--a 12 percent gain. This earnings impact was due to a combination of greater hours of work and higher earnings per hour. Importantly, the quarterly earnings impacts in year 4 remained fairly constant and were each statistically significant at the 1 percent level (that is, the impacts *persisted* in year 4).

Over the whole period, the average earnings of Job Corps participants were $624 higher than they would otherwise have been, although this impact is not statistically significant. This impact is small because it took about two years from random assignment for the earnings of the program group to reach those of the control group, a consequence of the substantial time participants invested in their education and training.

Positive impacts during the 48-month follow-up period were found broadly across subgroups of youths defined by their characteristics at random assignment. The program provided gains for males, females with and without children, very young students, older youths with and without a high school credential, and whites and African Americans (but not Hispanics).

For those assigned to the residential component, postprogram earnings and employment impacts were positive overall. Impacts were similar for males, females with children, and females without children. Thus, the residential program component was effective for broad groups of students.

Earnings and employment impacts were also positive overall for nonresidential designees. Substantial earnings gains were found for females with children and males, but no impacts were evident for nonresidential females without children.

**Job Corps had small beneficial impacts on the receipt of public assistance.** Overall, program group members reported receiving about $460 less in benefits (across several public assistance programs) than control group members. However, impacts on the receipt of individual types of assistance were small and in many cases not statistically significant. For example, the typical program group member received AFDC/TANF benefits for just 0.4 months less than the typical control group member (5.0 months, compared to 5.4 months for the control group), and received food stamp benefits for just 0.5 months less (6.5 months, compared to 7.0 months).

**Job Corps significantly reduced participants' involvement with the criminal justice system.** The arrest rate was reduced by 16 percent (about 5 percentage points). Arrest rate reductions were largest during the first year after random assignment, when most program group enrollees were in Job Corps. However, Job Corps also led to small arrest reductions during the later months of the follow-up period, after most of the program group had left the program. Furthermore, although the level of arrest rates differed substantially across subgroups, the impacts on arrest rates were very similar across subgroups (although no effects were found for male nonresidential designees).

Program group members were less likely to have arrest charges for nearly all categories of crimes. However, reductions were slightly larger for less serious crimes (such as disorderly conduct and trespassing).

Job Corps participation also reduced convictions and incarcerations resulting from a conviction. More than 25 percent of control group members were ever convicted during the follow-up period, compared to 22 percent of program group members. Similarly, Job Corps participation reduced the percentage incarcerated for convictions by 2 percentage points (from 18 percent to 16 percent).

Job Corps participation also led to reductions in crimes committed against program participants. The frequency of victimizations was reduced most during the in-program period, but the reductions persisted somewhat afterwards.

**Job Corps had small positive impacts on self-assessed health status, independent living, and the use of child care, but none on self-reported illegal drug use, fertility, or custodial responsibility.** Job Corps had little effect on the self-reported use of tobacco, alcohol, and illegal drugs, for the full sample and for key subgroups. It also had little effect on time spent in drug treatment.

Job Corps significantly reduced the percentage of youth who rated their health as "poor" or "fair" at the time of the 12-, 30-, and 48-month interviews. At each interview, about 17.5 percent of the control group and 15.5 percent of the program group said their health was "poor" or "fair."

The program had no effect on fertility or custodial responsibility, either for the full sample or by gender. About 38 percent of those in both the program and control groups had a child during the follow-up period (49 percent of females and 31 percent of males), and more than 80 percent of children were born out of wedlock. About two-thirds of all parents (and 42 percent of male parents) were living with all their children, and about 82 percent of male parents provided support for noncustodial children.

Job Corps participation, however, did have a small effect on promoting independent living at the 48-month interview point. A slightly smaller percentage of program group members were living with their parents (32 percent, compared to 35 percent of control group members), and a slightly larger percentage were living, either married or unmarried, with a partner (31 percent, compared to 29 percent). Furthermore, the average distance between the zip codes of residence at application to Job Corps and at the 48-month interview was slightly larger for the program group. The same pattern holds for males and females with and without children at baseline.

Finally, Job Corps participation led to increases in the use of child care. Participants used an average of about 146 more hours of child care during the 48-month period than they would have otherwise. Impacts on child care use were positive during the first year after random assignment (when many program group members were enrolled in Job Corps) and during the fourth year (when employment impacts were the largest).

## B. CONCLUDING OBSERVATIONS

Job Corps provides a residential living program, health care, and a broad range of services designed to help youth who have not succeeded in school to become productive young adults. Many staff and observers of the program believe that the distinctive residential component of Job Corps is a key ingredient, both because it is necessary for delivering effective academic and vocational instruction and because the experience of living in a community committed to learning has intrinsic benefits apart from the formal education and training that Job Corps provides.

Because of the comprehensive nature of Job Corps, it is not possible to determine precisely the relative contributions of the different parts of the program to the beneficial impacts that we find. We can, however, put the postprogram earnings gains into perspective by using the literature on the returns to schooling, and our findings that (1) youths who enroll in Job Corps receive the equivalent of nearly a full year of schooling that they would not have received if Job Corps were not available to them, and (2) the vast majority who leave school to go to Job Corps would have dropped out and not obtained a high school credential had they not enrolled in the program.

Economists have long been concerned about the returns to schooling. They pose the question, How much difference does an additional year of schooling make in the lifetime earnings of an individual? The answers they have developed over the past two decades provide an important perspective on the study's findings.

Studies of the average returns to a year of schooling consistently find that a year of schooling increases earnings over a worker's lifetime by 8 to 12 percent.[1] Measured in hours spent in education and training, Job Corps provides roughly the equivalent of a year of additional schooling per participant. In this context, the 12 percent earnings gains and the persistence of the earnings gains during the latter part of the 48-month period are in line with what one would expect from an intensive education and training program that serves primarily school-aged youth.

It is also noteworthy that no other studied education and training program for disadvantaged youth has produced statistically significant earnings and employment gains. For example, the National JTPA Study found no impacts over a 30-month period on the earnings of low-income out-of-school youths who participated in 15 selected JTPA Title II-A programs in the late 1980s (Orr et al. 1996).[2] As another example, the JOBSTART demonstration, conducted in 13 local areas, provided education, training, and job placement services in a nonresidential setting to economically disadvantaged dropouts ages 17 to 21. The profiles of earnings and earnings gains were similar over a four-year follow-up period to the gains reported here for Job Corps.[3] However, the gains were not statistically significant (Cave et al. 1993).[4] Thus, Job Corps is the only program that has produced sustained and statistically significant earnings gains.

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[1] See Tables 4 and 5 in Card (1999).

[2] The study used a random assignment design where more than 5,500 youths between the ages of 16 and 21 were randomly assigned to a research status.

The finding that Job Corps improves key outcomes for broad groups of students rather than for only a subset provides further evidence that the program is effective. Participation led to substantial improvements in education-related outcomes for all subgroups of students that we investigated. Employment and earnings gains were similar for males and females. Postprogram earnings gains were found for groups of students at special risk of poor outcomes (such as very young students, females with children, those arrested for nonserious crimes, and older youths who did not possess a high school credential at baseline) *as well as* for groups at lower risk (such as older students with a high school credential at baseline). The program increased earnings for whites as well as for African Americans (although earnings gains were not found for Hispanics), and for those who applied before and after the ZT policies took effect. Reductions in criminal activity were found for nearly all groups of students. Finally, beneficial impacts for key outcomes were found broadly across regions and for different types of centers (as discussed in Burghardt et al. 2001). Thus, Job Corps effectively serves a broad group of students with differing abilities and needs.

While Job Corps is broadly effective, the impacts for several particularly vulnerable or difficult-to-serve groups are especially noteworthy. First, beneficial program impacts were found for 16- and 17-year-old youth. For this group: (1) average earnings gains per participant were nearly $900 in year 4, (2) the percentage earning a high school diploma or GED was up by 66 percent, and (3) arrest rates were reduced by 11 percent and rates of incarceration for a conviction by 19 percent. While staff find this age group difficult to deal with, and while more of them leave Job Corps before completing their education and training than do older students, they do appear to benefit from their program experiences.

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[3] The impact on annual earnings per eligible applicant in JOBSTART was $423 in follow-up year 3 and $410 in follow-up year 4, approximately 9 and 8 percent, respectively, of the control group's mean earnings. (Cave et al. 1993, Table 5.1).

[4] The sample for the JOBSTART random assignment evaluation contained about 1,000 program group members and 1,000 control group members.

Second, females with children at the time of enrollment enjoyed significant earnings gains and modest reductions in welfare receipt. More than one-half of young women with children enrolled in Job Corps as nonresidential students, because child-rearing responsibilities required that they live at home. However, these young women received similar amounts of academic classroom instruction and vocational training as other students, despite the fact that many lived at home. Furthermore, they enjoyed increases of more than 20 percent in their earnings and reductions of about 12 percent in the receipt of public assistance near the end of the 48-month follow-up period.

Our findings suggest that both the residential and the nonresidential program components are effective for the students they serve. Impacts on earnings during the postprogram period were positive for five of the six subgroups defined by residential designation status, gender, and the presence of children at baseline for females. Yet, it is *not* appropriate to conclude that the residential component could be abolished and everyone served just as well in the less expensive nonresidential component, for several important reasons. First, the two components serve very different students. Nonresidential students tend to be females with children and older youths who would be unable to participate in the residential Job Corps program because of family responsibilities. Residential students, on the other hand, tend to be younger and less educated, and are deemed by Job Corps staff to require training in a residential setting in order to benefit fully from the program. Consequently, our results cannot be used to assess how students in the residential component would fare in the nonresidential component. Second, most centers with nonresidential slots also have residential slots, so nearly all nonresidential students train with residential students and may benefit from interacting with them. Their program experiences would probably be much different without the residential component. Finally, nonresidential students receive services that are similar in many ways to those received by residential students, and the nonresidential component of Job Corps is more intensive and comprehensive than most other nonresidential training programs. In fact, the program cost per nonresidential student is only about 16 percent less than the program cost per residential student (McConnell et al. 2001). Thus, the cost of Job Corps would not be reduced significantly if all students were served in the nonresidential component.

In conclusion, we find that Job Corps produces beneficial impacts on the main outcomes that it intends to influence. Beneficial impacts on education-related, employment-related, and crime-related outcomes were found for the full population of students as well as for broad subgroups. The residential and nonresidential program components were each effective for the students they served. A companion report, presenting findings from the benefit-cost analysis, concludes that Job Corps is a worthwhile investment both for the students and for the broader society that supports their efforts.

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# APPENDIX A

# SUBGROUP SAMPLE SIZES

## TABLE A.1

## SUBGROUP SAMPLE SIZES FOR THE 48-MONTH SAMPLE

| | | Program Group | | |
|---|---|---|---|---|
| Subgroup | Control Group | Full Sample | Job Corps Participants | Percentage of Study Population |
| Gender | | | | |
| Male | 2,787 | 3,741 | 2,799 | 59.4 |
| Female | 1,698 | 3,087 | 2,126 | 40.6 |
| Missing | 0.0 | 0.0 | 0.0 | |
| Age at Application | | | | |
| 16 to 17 | 1,907 | 2,742 | 2,132 | 41.2 |
| 18 to 19 | 1,402 | 2,175 | 1,518 | 32.0 |
| 20 to 24 | 1,176 | 1,911 | 1,275 | 26.8 |
| Missing | 0.0 | 0.0 | 0.0 | |
| Educational Attainment at Random Assignment | | | | |
| Had a high school diploma | 798 | 1,316 | 887 | 18.3 |
| Had a GED | 230 | 310 | 209 | 4.8 |
| Had neither | 3,436 | 5,161 | 3,800 | 77.0 |
| Missing | 21 | 41 | 29 | |
| Presence of Children at Random Assignment for Females | | | | |
| Had children | 538 | 1,005 | 637 | 28.7 |
| Had no children | 1,146 | 2,060 | 1,477 | 71.3 |
| Missing | 14 | 22 | 12 | |
| Arrest History at Random Assignment | | | | |
| Never arrested | 3,225 | 5,020 | 3,692 | 76.6 |
| Ever arrested for nonserious crimes only[a] | 795 | 1,158 | 812 | 18.7 |
| Ever arrested for serious crimes[a] | 203 | 294 | 211 | 4.7 |
| Missing[b] | 262 | 356 | 210 | |
| Race | | | | |
| White, non-Hispanic | 1,193 | 1,793 | 1,257 | 27.0 |
| Black, non-Hispanic | 2,179 | 3,366 | 2,454 | 47.4 |
| Hispanic | 787 | 1,175 | 851 | 17.7 |
| Other | 326 | 494 | 363 | 7.9 |
| American Indian or Alaskan Native | 177 | 248 | 185 | 4.1 |
| Asian or Pacific Islander | 82 | 129 | 95 | 2.2 |
| Other | 67 | 117 | 83 | 1.6 |
| Missing | 0.0 | 0.0 | 0.0 | |

TABLE A.1 *(continued)*

| | | Program Group | | |
|---|---|---|---|---|
| Subgroup | Control Group | Full Sample | Job Corps Participants | Percentage of Study Population |
| Job Corps Application Date and the New Job Corps Policies | | | | |
| Prior to 3/1/95 (before ZT) | 986 | 1,622 | 1,141 | 22.3 |
| On or after 3/1/95 (after ZT) | 3,499 | 5,206 | 3,784 | 77.7 |
| Missing | 0.0 | 0.0 | 0.0 | |
| Residential Designation Status | | | | |
| Residential designees | 3,753 | 5,484 | 4,057 | 86.0 |
| Males | 2,581 | 3,373 | 2,542 | 55.3 |
| Females without children | 957 | 1,710 | 1,249 | 25.3 |
| Females with children | 206 | 387 | 257 | 5.4 |
| Nonresidential designees | 732 | 1,344 | 868 | 14.0 |
| Males | 206 | 368 | 257 | 4.2 |
| Females without children | 189 | 350 | 228 | 3.6 |
| Females with children | 332 | 618 | 380 | 6.2 |
| Missing | 0.0 | 0.0 | 0.0 | |
| **Sample Size** | **4,485** | **6,828** | **4,925** | **80,883** |

SOURCE: Baseline interview data and ETA-652 Supplement data.

[a] Serious crimes include murder, assault, robbery, and burglary. Nonserious crimes include larceny, vehicle theft, other property crimes, drug law violations, other personal crimes, and other miscellaneous crimes.

[b] Crime information was not collected for those who completed the abbreviated baseline interview at the end of the 12-month interview. These youths were administered this interview because they did not complete a full baseline interview.

## APPENDIX B

## SUPPLEMENTARY TABLES TO CHAPTER IV

## TABLE B.1

## QUARTERLY ENROLLMENT RATES IN JOB CORPS FOR PROGRAM GROUP MEMBERS
(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Enrolled in a Job Corps Center | 73.2 | 75.8 | 69.6 | 64.1 | 78.8 | 70.6 | 67.9 |
| Job Corps Participation Rates, by Quarter | | | | | | | |
| 1 | 67.0 | 68.9 | 64.2 | 57.8 | 72.0 | 64.7 | 62.0 |
| 2 | 52.3 | 53.3 | 50.9 | 44.1 | 55.0 | 50.6 | 50.2 |
| 3 | 38.4 | 38.6 | 38.0 | 31.2 | 38.1 | 37.2 | 40.1 |
| 4 | 27.4 | 27.7 | 27.0 | 21.7 | 26.4 | 26.1 | 30.3 |
| 5 | 21.2 | 21.7 | 20.6 | 16.9 | 21.2 | 19.5 | 23.4 |
| 6 | 13.7 | 13.6 | 13.9 | 11.8 | 13.5 | 12.3 | 15.8 |
| 7 | 8.9 | 9.1 | 8.7 | 7.6 | 8.6 | 7.9 | 10.5 |
| 8 | 5.9 | 5.7 | 6.2 | 5.9 | 5.1 | 5.5 | 7.7 |
| 9 | 4.3 | 4.2 | 4.3 | 4.3 | 3.7 | 4.4 | 5.0 |
| 10 | 3.0 | 2.8 | 3.2 | 2.8 | 2.5 | 3.3 | 3.2 |
| 11 | 2.4 | 2.3 | 2.5 | 1.5 | 2.1 | 2.7 | 2.4 |
| 12 | 1.7 | 1.6 | 1.8 | 1.3 | 1.5 | 1.9 | 1.6 |
| 13 | 1.3 | 1.3 | 1.3 | 0.7 | 1.4 | 1.3 | 1.2 |
| 14 | 0.8 | 0.9 | 0.7 | 0.4 | 1.1 | 0.8 | 0.5 |
| 15 | 0.8 | 0.9 | 0.7 | 0.4 | 1.0 | 0.8 | 0.4 |
| 16 | 0.6 | 0.6 | 0.7 | 0.5 | 0.9 | 0.5 | 0.3 |
| Enrolled at 48 Months | 0.3 | 0.3 | 0.4 | 0.3 | 0.4 | 0.3 | 0.1 |
| **Sample Size** | **6,828** | **3,741** | **3,087** | **1,005** | **2,742** | **2,175** | **1,911** |

SOURCE: 12-, 30-, and 48-month follow-up interview and SPAMIS data for those who completed 48-month interviews.

NOTE: Data pertain to program group members in the research sample. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

TABLE B.2

## PARTICIPATION IN OTHER JOB CORPS ACTIVITIES FOR PROGRAM GROUP ENROLLEES

(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| Activity or Program | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Progress/Performance Evaluation Panels (P/PEPs) | 81.6 | 80.6 | 83.1 | 80.5 | 80.2 | 82.0 | 83.7 |
| World of Work (WOW) | 76.5 | 75.4 | 78.1 | 73.2 | 74.8 | 78.4 | 77.1 |
| Social Skills Training (SST) | 75.5 | 75.6 | 75.4 | 69.8 | 74.9 | 74.3 | 78.0 |
| Health Classes | 74.4 | 74.8 | 73.7 | 70.5 | 73.4 | 74.8 | 75.6 |
| Cultural Awareness Classes | 64.7 | 63.0 | 67.3 | 64.5 | 61.1 | 66.4 | 68.9 |
| Parenting Skills Classes | 63.2 | 62.3 | 64.6 | 63.9 | 62.0 | 62.1 | 66.7 |
| Alcohol and Other Drugs of Abuse Program (AODA) | 47.8 | 49.0 | 46.1 | 44.0 | 48.4 | 47.8 | 46.8 |
| **Sample Size** | **4,925** | **2,799** | **2,126** | **637** | **2,132** | **1,518** | **1,275** |

SOURCE: 12- and 30-month follow-up interview data for those who completed 48-month interviews.

NOTE: Data pertain to program group members who enrolled in a Job Corps center during the 30 months after random assignment. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Questions on these Job Corps activities were not asked in the 48-month interview. Thus, these figures pertain to those in the analysis sample who completed 30-month interviews.

TABLE B.3

## JOB PLACEMENT SERVICES FOR PROGRAM GROUP ENROLLEES
(Percentages)

| | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|
| | Total | All Males | All Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Got Help Looking for a Job from Job Corps Staff or a Job Corps Placement Contractor | 39.7 | 39.3 | 40.3 | 37.2 | 39.8 | 37.9 | 41.6 |
| Type of Job Placement Services Received[a] | | | | | | | |
| Aptitude or skills assessment | 45.4 | 47.4 | 42.4 | 45.5 | 43.5 | 44.7 | 49.3 |
| Resume-writing assistance | 54.1 | 51.7 | 57.6 | 59.3 | 51.3 | 55.9 | 56.6 |
| Developing interviewing skills | 58.9 | 57.2 | 61.3 | 59.6 | 56.7 | 60.0 | 61.3 |
| Job search training | 58.2 | 57.4 | 59.3 | 62.1 | 57.4 | 58.1 | 59.6 |
| Career and job counseling | 41.2 | 39.1 | 44.2 | 49.5 | 37.4 | 42.2 | 46.2 |
| Job clubs or job banks | 18.3 | 17.2 | 19.8 | 15.8 | 17.5 | 18.4 | 19.4 |
| Direct job referral | 48.2 | 48.3 | 48.2 | 52.2 | 43.0 | 52.5 | 52.2 |
| Relocation assistance | 26.0 | 27.3 | 24.2 | 18.0 | 24.8 | 27.0 | 26.9 |
| Aid in enrolling in other training or education programs | 17.0 | 16.5 | 17.8 | 15.9 | 18.0 | 15.8 | 16.9 |
| Aid in joining the military | 12.7 | 13.8 | 11.1 | 8.6 | 12.5 | 13.1 | 12.6 |
| Other | 26.5 | 28.8 | 23.1 | 17.8 | 26.5 | 26.1 | 26.9 |
| Got a Job as a Result of the Job Placement Services Received[a] | 41.4 | 44.5 | 37.0 | 44.2 | 38.8 | 39.8 | 47.7 |
| **Sample Size** | **4,925** | **2,799** | **2,126** | **637** | **2,132** | **1,518** | **1,275** |

SOURCE: 12- and 30-month follow-up interview data for those who completed 48-month interviews.

NOTE: Data pertain to program group members who enrolled in and left a Job Corps center during the 30 months after random assignment. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Questions on these Job Corps activities were not asked in the 48-month interview. Thus, these figures pertain to those in the analysis sample who completed 30-month interviews.

[a] Data pertain to those who received help looking for a job from Job Corps staff or a Job Corps placement contractor.

## TABLE B.4

## STUDENTS' ASSESSMENT OF OTHER JOB CORPS ACTIVITIES FOR PROGRAM GROUP ENROLLEES

(Percentages)

| | | | Gender | | | Age | | |
|---|---|---|---|---|---|---|---|---|
| Program or Activity | Extent to Which Program Was Beneficial | Total | Males | Females | Females with Children | 16 to 17 | 18 to 19 | 20 to 24 |
| Progress/Performance Evaluation Panels (P/PEPs) | | | | | | | | |
| | A lot | 61.2 | 58.6 | 65.1 | 64.7 | 58.2 | 61.2 | 66.3 |
| | A little | 30.3 | 32.5 | 27.0 | 26.0 | 33.2 | 30.1 | 25.4 |
| | Not at all | 8.5 | 8.9 | 7.9 | 9.3 | 8.6 | 8.6 | 8.3 |
| World of Work (WOW) | | | | | | | | |
| | A lot | 55.6 | 53.7 | 58.4 | 62.3 | 56.8 | 54.8 | 54.5 |
| | A little | 34.0 | 35.1 | 32.4 | 28.8 | 34.7 | 34.9 | 31.8 |
| | Not at all | 10.4 | 11.2 | 9.2 | 8.8 | 8.5 | 10.2 | 13.7 |
| Social Skills Training (SST) | | | | | | | | |
| | A lot | 58.9 | 55.7 | 63.7 | 63.1 | 58.8 | 57.5 | 60.6 |
| | A little | 31.0 | 33.6 | 27.0 | 28.8 | 31.6 | 32.0 | 28.9 |
| | Not at all | 10.1 | 10.6 | 9.3 | 8.1 | 9.6 | 10.4 | 10.5 |
| Health Classes | | | | | | | | |
| | A lot | 59.6 | 57.1 | 63.7 | 64.8 | 60.6 | 57.0 | 61.1 |
| | A little | 31.3 | 32.9 | 28.8 | 28.7 | 30.7 | 33.2 | 30.0 |
| | Not at all | 9.1 | 10.1 | 7.5 | 6.5 | 8.6 | 9.7 | 8.9 |
| Cultural Awareness Classes | | | | | | | | |
| | A lot | 60.4 | 57.4 | 64.6 | 62.8 | 58.4 | 60.0 | 63.8 |
| | A little | 31.9 | 34.2 | 28.5 | 28.5 | 34.2 | 31.4 | 29.0 |
| | Not at all | 7.8 | 8.3 | 6.9 | 8.7 | 7.4 | 8.7 | 7.3 |
| Parenting Skills Classes | | | | | | | | |
| | A lot | 57.5 | 55.7 | 60.1 | 56.5 | 56.4 | 58.2 | 58.5 |
| | A little | 32.7 | 34.9 | 29.6 | 30.5 | 33.9 | 32.0 | 31.7 |
| | Not at all | 9.8 | 9.4 | 10.4 | 13.0 | 9.7 | 9.9 | 9.8 |
| Alcohol and Other Drugs of Abuse Program (AODA) | | | | | | | | |
| | A lot | 59.5 | 55.9 | 65.9 | 64.7 | 58.6 | 58.7 | 62.1 |
| | A little | 25.8 | 28.0 | 21.9 | 24.5 | 25.2 | 25.8 | 27.1 |
| | Not at all | 14.7 | 16.2 | 12.2 | 10.8 | 16.2 | 15.5 | 10.8 |
| **Sample Size** | | **4,925** | **2,799** | **2,126** | **637** | **2,132** | **1,518** | **1,275** |

SOURCE: 12- and 30-month follow-up interview data for those who completed 48-month interviews.

NOTE: Data pertain to program group members who took the specified classes or participated in the specified programs. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Questions on these Job Corps activities were not asked in the 48-month interview. Thus, these figures pertain to those in the analysis sample who completed 30-month interviews.

## TABLE B.5

## JOB CORPS EXPERIENCES, BY RESIDENTIAL STATUS AND GENDER

| | Enrollment Rate (Percentage) | Length of Stay in Job Corps (Months) | In Job Corps Less than Three Months (Percentage) | In Job Corps More than 12 Months (Percentage) | Average Hours in Academic Classes | Average Hours in Vocational Training | Average Hours in Academic Classes or Vocational Training |
|---|---|---|---|---|---|---|---|
| Residential Designees | | | | | | | |
| All residents | 74.5 | 8.0 | 28.7 | 23.7 | 444 | 713 | 1,156 |
| Males | 76.2 | 7.9 | 29.9 | 22.8 | 427 | 714 | 1,141 |
| Females without children | 72.8 | 8.7 | 25.3 | 27.0 | 491 | 736 | 1,227 |
| Females with children | 66.4 | 6.9 | 32.3 | 18.1 | 399 | 570 | 969 |
| Nonresidential Designees | | | | | | | |
| All residents | 65.6 | 8.1 | 25.8 | 22.7 | 405 | 624 | 1,028 |
| Males | 70.9 | 7.3 | 30.5 | 20.1 | 390 | 578 | 969 |
| Females without children | 65.4 | 8.5 | 23.6 | 24.5 | 423 | 639 | 1,062 |
| Females with children | 62.0 | 8.4 | 23.7 | 23.4 | 404 | 646 | 1,051 |

SOURCE: Baseline and 12-, 30, and 48-month follow-up interview data and SPAMIS data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

## TABLE B.6

## EXPERIENCES IN JOB CORPS, BY HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE, AND APPLICATION DATE

| Subgroup | Enrollment Rate (Percentage) | Average Length of Stay in Job Corps (Months) | In Job Corps Less than Three Months (Percentage) | In Job Corps More than 12 Months (Percentage) | Average Hours in Academic Classes | Average Hours in Vocational Training | Average Hours in Academic Classes or Vocational Training |
|---|---|---|---|---|---|---|---|
| Educational Attainment at Random Assignment and Age at Application | | | | | | | |
| Had high school diploma or GED | 68.5 | 9.1 | 23.8 | 27.8 | 256 | 922 | 1,178 |
| Age 16 to 17 | 67.7 | 8.0 | 19.7 | 23.2 | 354 | 709 | 1,063 |
| Age 18 to 19 | 70.7 | 8.9 | 22.2 | 25.7 | 230 | 904 | 1,134 |
| Age 20 to 24 | 67.1 | 9.4 | 25.2 | 29.6 | 266 | 951 | 1,217 |
| Had no high school credential | 74.7 | 7.7 | 29.6 | 22.4 | 491 | 637 | 1,127 |
| Age 16 to 17 | 79.1 | 7.4 | 31.0 | 21.5 | 486 | 610 | 1,096 |
| Age 18 to 19 | 70.6 | 7.6 | 30.3 | 21.6 | 451 | 637 | 1,088 |
| Age 20 to 24 | 68.4 | 8.9 | 23.3 | 26.7 | 578 | 728 | 1,306 |
| Arrest History at Random Assignment | | | | | | | |
| Never arrested | 74.9 | 8.4 | 27.0 | 25.7 | 463 | 734 | 1,197 |
| Ever arrested for nonserious crimes only | 71.3 | 6.9 | 32.5 | 17.7 | 351 | 608 | 959 |
| Ever arrested for serious crimes[a] | 73.5 | 7.0 | 30.1 | 15.4 | 378 | 565 | 943 |
| Race and Ethnicity | | | | | | | |
| White non-Hispanic | 71.5 | 7.6 | 30.5 | 21.5 | 310 | 753 | 1,063 |
| Black non-Hispanic | 74.1 | 7.7 | 29.8 | 21.5 | 454 | 630 | 1,084 |
| Hispanic | 73.1 | 9.4 | 20.9 | 30.2 | 563 | 787 | 1,351 |
| Other[b] | 74.4 | 8.5 | 28.0 | 28.7 | 525 | 763 | 1,288 |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | |
| Prior to 3/1/95 (before ZT) | 71.7 | 8.0 | 28.7 | 24.1 | 447 | 685 | 1,132 |
| On or after 3/1/95 (after ZT) | 73.7 | 8.1 | 28.2 | 23.4 | 437 | 705 | 1,142 |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview and SPAMIS data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse.

[a] Serious crimes include aggravated assault, murder, robbery, and burglary.

[b] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

# APPENDIX C

# SUPPLEMENTARY TABLES TO CHAPTER V

## TABLE C.1

## IMPACTS ON TIME SPENT IN EDUCATION AND TRAINING PROGRAMS, BY TYPE OF PROGRAM

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Percentage of Weeks Ever in Education or Training, by Type of Program | | | | | | |
| Job Corps | 11.5 | 0.4 | 11.0*** | 15.6 | 15.3*** | |
| Programs other than Job Corps | 12.7 | 17.7 | -5.0*** | 11.4 | -6.9*** | -37.8 |
| ABE[d] | 0.6 | 0.9 | -0.3*** | 0.5 | -0.4*** | -41.7 |
| ESL[d] | 0.1 | 0.2 | -0.1 | 0.1 | -0.1 | -42.0 |
| GED[d] | 4.1 | 5.7 | -1.6*** | 3.4 | -2.3*** | -39.7 |
| High school[d] | 3.0 | 6.1 | -3.1*** | 2.4 | -4.3*** | -64.2 |
| Vocational, technical, or trade school | 3.4 | 4.0 | -0.5*** | 3.1 | -0.8*** | -19.8 |
| Two-year college | 2.5 | 3.0 | -0.5** | 2.3 | -0.7** | -22.8 |
| Four-year college | 0.8 | 0.8 | 0.0 | 0.7 | 0.1 | 9.6 |
| Other | 0.3 | 0.3 | -0.1* | 0.2 | -0.1* | -30.6 |
| Average Hours per Week Ever in Education or Training, by Type of Program | | | | | | |
| Job Corps | 4.6 | 0.2 | 4.4*** | 6.3 | 6.1*** | |
| Programs other than Job Corps | 2.8 | 4.1 | -1.2*** | 2.5 | -1.7*** | -40.0 |
| ABE[d] | 0.1 | 0.2 | -0.1*** | 0.1 | -0.1*** | -46.1 |
| ESL[d] | 0.02 | 0.03 | -0.02** | 0.02 | -0.02** | -60.4 |
| GED[d] | 0.7 | 0.9 | -0.3*** | 0.6 | -0.4*** | -38.9 |
| High school[d] | 0.9 | 1.8 | -0.9*** | 0.7 | -1.2*** | -63.9 |
| Vocational, technical, or trade school | 0.9 | 1.0 | -0.1*** | 0.8 | -0.2*** | -18.2 |
| Two-year college | 0.5 | 0.6 | -0.1** | 0.4 | -0.1** | -20.9 |
| Four-year college | 0.2 | 0.2 | 0.0 | 0.2 | 0.0 | -2.0 |
| Other | 0.1 | 0.1 | 0.0 | 0.0 | 0.0 | -28.8 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] Figures pertain to sample members who did not have a high school credential at baseline.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

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## TABLE C.2

## TIME SPENT IN EDUCATION AND TRAINING PROGRAMS FOR THOSE ENROLLED IN TYPE OF PROGRAM

| Outcome Measure | Program Group | Control Group | Difference[a] |
|---|---|---|---|
| Average Percentage of Weeks in Education or Training for Those Enrolled in Type of Program (Percentage) | | | |
| Programs other than Job Corps | 20.2 | 24.9 | -4.7*** |
| ABE/ESL[b] | 9.6 | 11.6 | -2.0** |
| GED[b] | 13.2 | 13.7 | -0.5 |
| High school[b] | 13.5 | 19.4 | -5.9*** |
| Vocational, technical, or trade school | 13.2 | 14.0 | -0.8 |
| Two-year college | 21.9 | 24.3 | -2.5** |
| Four-year college | 25.9 | 23.9 | 2.0 |
| Other | 9.2 | 8.3 | 0.9 |
| Average Hours per Week in Education or Training for Those Enrolled in Type of Program | | | |
| Programs other than Job Corps | 4.5 | 5.7 | -1.2*** |
| ABE/ESL[b] | 1.7 | 2.2 | -0.5** |
| GED[b] | 2.1 | 2.2 | -0.1 |
| High school[b] | 3.9 | 5.6 | -1.7*** |
| Vocational, technical, or trade school | 3.3 | 3.5 | -0.2 |
| Two-year college | 4.2 | 4.6 | -0.4 |
| Four-year college | 5.5 | 5.4 | 0.1 |
| Other | 1.8 | 1.6 | 0.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Because these estimates are conditional on enrollment, they are not impact estimates.

[b] Data pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE C.3

## TYPES OF PROGRAMS RECEIVED ACADEMIC CLASSROOM INSTRUCTION AND VOCATIONAL TRAINING

| Outcome Measure | Program Group | Control Group | Difference[a] |
|---|---|---|---|
| Places Ever Took Academic Classes (for Those Who Took Any Classes) | | | |
| Job Corps | 76.3 | 7.1 | 69.2*** |
| Programs other than Job Corps | 23.6 | 92.8 | -69.2*** |
| High school/GED or ABE | 14.2 | 67.4 | -53.1*** |
| Vocational, technical, or trade school | 4.4 | 16.7 | -12.3*** |
| Two-year college | 5.5 | 16.3 | -10.8*** |
| Four-year college | 1.1 | 2.3 | -1.2*** |
| Other | 4.0 | 15.9 | -11.9*** |
| Places Ever Received Vocational Training (for Those Who Received Any Training) | | | |
| Job Corps | 87.0 | 13.4 | 73.6*** |
| Programs other than Job Corps | 12.6 | 84.2 | -71.6*** |
| High school/GED or ABE | 1.9 | 16.7 | -14.8*** |
| Vocational, technical, or trade school | 10.0 | 63.4 | -53.4*** |
| Two-year college | 1.8 | 11.3 | -9.5*** |
| Four-year college | 0.2 | 0.3 | 0.0 |
| Other | 0.2 | 3.0 | -2.8*** |
| **Sample Size** | **6,828** | **4,485** | **11,313** |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Because these estimates are conditional on enrollment, they are not impact estimates.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE C.4

## IMPACTS ON EDUCATION AND TRAINING OUTCOMES FOR 16- AND 17-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Ever Enrolled in a Program During the 48 Months After Random Assignment*** | 95.9 | 82.5 | 13.4*** | 100.0 | 17.2*** | 20.8 |
| Percentage Enrolled in a Program, by Quarter After Random Assignment | | | | | | |
| 1*** | 83.7 | 43.6 | 40.1*** | 95.5 | 51.7*** | 118.1 |
| 2*** | 71.3 | 45.5 | 25.8*** | 80.5 | 33.2*** | 70.2 |
| 3*** | 59.2 | 44.8 | 14.4*** | 64.8 | 18.5*** | 40.0 |
| 4*** | 50.5 | 44.9 | 5.6*** | 53.3 | 7.2*** | 15.7 |
| 5*** | 43.8 | 41.0 | 2.8* | 46.2 | 3.6* | 8.4 |
| 6*** | 34.2 | 33.9 | 0.3 | 35.3 | 0.4 | 1.2 |
| 7*** | 29.5 | 29.8 | -0.4 | 30.0 | -0.5 | -1.6 |
| 8*** | 25.7 | 26.9 | -1.2 | 25.4 | -1.6 | -5.8 |
| 9** | 24.0 | 25.2 | -1.2 | 23.1 | -1.5 | -6.3 |
| 10 | 21.8 | 23.2 | -1.4 | 21.0 | -1.9 | 95.9 |
| 11 | 22.2 | 22.3 | -0.1 | 21.6 | -0.1 | -0.4 |
| 12 | 20.4 | 20.1 | 0.3 | 19.9 | 0.3 | 1.7 |
| 13 | 18.0 | 18.1 | -0.1 | 17.1 | -0.2 | -1.0 |
| 14 | 16.6 | 18.3 | -1.7 | 15.8 | -2.3 | -12.5 |
| 15 | 16.4 | 17.8 | -1.4 | 15.9 | -1.8 | -10.0 |
| 16 | 17.9 | 17.7 | 0.1 | 16.8 | 0.2 | 1.0 |
| Average Percentage of Weeks Ever in Education or Training*** | 0.3 | 0.2 | 0.0*** | 0.3 | 0.0*** | 20.6 |
| Average Hours per Week Ever in Education or Training*** | 8.1 | 5.5 | 2.6*** | 8.8 | 3.3*** | 60.9 |
| Type of Programs Other than Job Corps Ever Attended | | | | | | |
| Any program*** | 71.0 | 82.3 | -11.3*** | 68.5 | -14.5*** | -17.5 |
| ABE or ESL**[e] | 7.2 | 9.5 | -2.2*** | 6.8 | -2.9*** | -29.9 |
| GED***[e] | 34.0 | 46.5 | -12.4*** | 30.7 | -16.0*** | -34.3 |
| High school***[e] | 33.3 | 45.8 | -12.5*** | 31.6 | -16.1*** | -33.7 |
| Vocational, technical, or trade school | 23.0 | 25.6 | -2.6** | 22.5 | -3.4** | -13.0 |
| Two-year college | 8.3 | 8.7 | -0.5 | 8.1 | -0.6 | -6.8 |
| Four-year college | 2.2 | 2.0 | 0.2 | 2.2 | 0.3 | 12.9 |
| Other | 3.1 | 4.1 | -1.0* | 3.1 | -1.3* | -28.7 |
| Percentage Ever Took Academic Classes*** | 90.6 | 74.0 | 16.6*** | 97.3 | 21.3*** | 28.1 |
| Average Percentage of Weeks Ever in Academic Classes*** | 17.1 | 15.0 | 2.1*** | 18.1 | 2.7*** | 17.6 |
| Average Hours per Week in Academic Classes, by Year | | | | | | |
| All years*** | 3.8 | 3.7 | 0.1 | 4.0 | 0.1 | 3.8 |
| 1*** | 9.2 | 8.0 | 1.3*** | 10.0 | 1.7*** | 19.8 |
| 2*** | 4.2 | 4.8 | -0.6* | 4.4 | -0.7* | -14.7 |
| 3 | 1.8 | 1.9 | -0.1 | 1.7 | -0.2 | -8.8 |
| 4 | 0.5 | 0.6 | -0.1 | 0.5 | -0.1 | -20.3 |
| Percentage Ever Received Vocational Training*** | 77.4 | 27.8 | 49.6*** | 92.0 | 63.9*** | 227.1 |

TABLE C.4 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Percentage of Weeks Received Vocational Training | 13.0 | 4.1 | 8.9*** | 15.6 | 11.4*** | 274 |
| Average Hours per Week Received Vocational Training, by Year | | | | | | |
| All years | 2.9 | 0.8 | 2.1*** | 3.5 | 2.7*** | 358.3 |
| 1** | 6.9 | 0.8 | 6.1*** | 8.5 | 7.9*** | 1185.1 |
| 2 | 2.5 | 0.9 | 1.7*** | 3.0 | 2.2*** | 249.9 |
| 3 | 1.1 | 0.7 | 0.4** | 1.3 | 0.5** | 61.5 |
| 4 | 1.0 | 0.7 | 0.3* | 1.1 | 0.4* | 51.4 |
| Degrees, Diplomas, and Certificates Ever Received | | | | | | |
| GED certificate or high school diploma[e] | 46.7 | 36.2 | 10.6*** | 48.6 | 13.6*** | 38.9 |
| GED certificate[e] | 41.2 | 27.6 | 13.6*** | 43.9 | 17.5*** | 66.1 |
| High school diploma[e] | 5.5 | 8.5 | -3.0*** | 4.6 | -3.9*** | -45.8 |
| Vocational, technical, or trade certificate | 33.5 | 11.6 | 21.9*** | 39.2 | 28.2*** | 257.2 |
| College degree (two-year or four-year) | 0.4 | 0.6 | -0.2 | 0.4 | -0.3 | -37.3 |
| Average Highest Grade Completed at the 48-Month Interview** | | | | | | |
| Less than 9 | 10.9 | 9.1 | 1.8* | 11.1 | 2.3* | 26.3 |
| 9 to 11 | 73.4 | 73.7 | -0.3 | 74.4 | -0.4 | -0.5 |
| 12 | 12.8 | 14.8 | -2.0 | 11.9 | -2.6 | -18.1 |
| Greater than 12 | 2.9 | 2.4 | 0.5 | 2.6 | 0.7 | 36.8 |
| Average Highest Grade Completed** | 10.1 | 10.2 | -0.1** | 10.1 | -0.1** | -1.2 |
| **Sample Size** | **2,742** | **1,907** | **4,649** | **2,132** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to an outcome indicate the significance level of the statistical test for differences in the impacts across the three subgroups defined by age and high school credential status.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the proportion of program group members who enrolled in Job Corps. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] Figures pertain to those who did not have a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE C.5

### IMPACTS ON EDUCATION AND TRAINING OUTCOMES FOR 18- TO 24-YEAR-OLDS WITHOUT A HIGH SCHOOL CREDENTIAL AT RANDOM ASSIGNMENT

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Ever Enrolled in a Program During the 48 Months After Random Assignment*** | 90.9 | 67.6 | 23.3*** | 100.0 | 34.2*** | 51.6 |
| Percentage Enrolled in a Program, by Quarter After Random Assignment | | | | | | |
| 1*** | 72.0 | 21.3 | 50.6*** | 94.9 | 74.2*** | 358.2 |
| 2*** | 59.8 | 24.3 | 35.6*** | 78.1 | 52.1*** | 199.7 |
| 3*** | 48.8 | 23.9 | 25.0*** | 61.9 | 36.6*** | 144.2 |
| 4*** | 41.4 | 22.6 | 18.8*** | 49.7 | 27.5*** | 123.6 |
| 5*** | 35.2 | 20.9 | 14.3*** | 40.4 | 20.9*** | 107.7 |
| 6*** | 27.7 | 19.2 | 8.5*** | 30.7 | 12.5*** | 68.5 |
| 7*** | 23.4 | 17.5 | 5.9*** | 25.1 | 8.7*** | 53.0 |
| 8*** | 21.4 | 17.7 | 3.7*** | 21.7 | 5.4*** | 33 |
| 9** | 20.1 | 17.9 | 2.2* | 20.1 | 3.2* | 19.1 |
| 10 | 18.8 | 18.9 | -0.2 | 18.1 | -0.3 | -1.5 |
| 11 | 18.6 | 18.5 | 0.1 | 17.7 | 0.2 | 1.0 |
| 12 | 16.7 | 18.2 | -1.5 | 15.8 | -2.2 | -12.3 |
| 13 | 15.6 | 18.4 | -2.8** | 14.4 | -4.0** | -21.9 |
| 14 | 15.0 | 16.6 | -1.6 | 14.2 | -2.4 | -14.2 |
| 15 | 15.4 | 17.8 | -2.4** | 15.3 | -3.5** | -18.7 |
| 16 | 15.7 | 16.1 | -0.4 | 15.5 | -0.6 | -3.6 |
| Average Percentage of Weeks Ever in Education or Training*** | 0.2 | 0.1 | 0.1*** | 0.3 | 0.1*** | 77.0 |
| Average Hours per Week Ever in Education or Training*** | 6.9 | 3.0 | 3.9*** | 8.6 | 5.7*** | 197.5 |
| Type of Programs Other than Job Corps Ever Attended | | | | | | |
| Any program*** | 59.5 | 66.8 | -7.2*** | 55.2 | -10.6*** | -16.1 |
| ABE or ESL**[e] | 7.3 | 7.5 | -0.2 | 5.9 | -0.2 | -4.0 |
| GED***[e] | 27.3 | 36.2 | -9.0*** | 21.9 | -13.2*** | -37.6 |
| High school***[e] | 9.8 | 15.2 | -5.3*** | 9.0 | -7.8*** | -46.5 |
| Vocational, technical, or trade school | 24.4 | 26.1 | -1.7 | 22.7 | -2.5 | -9.8 |
| Two-year college | 9.1 | 9.3 | -0.2 | 9.8 | -0.3 | -2.6 |
| Four-year college | 1.8 | 1.8 | 0.0 | 2.1 | -0.1 | -2.7 |
| Other | 2.3 | 3.5 | -1.2** | 1.9 | -1.8** | -48.2 |
| Percentage Ever Took Academic Classes*** | 81.7 | 50.7 | 31.0*** | 93.6 | 45.4*** | 94.2 |
| Average Percentage of Weeks Ever in Academic Classes*** | 12.7 | 9.1 | 3.6*** | 14.6 | 5.3*** | 57.2 |
| Average Hours per Week in Academic Classes, by Year | | | | | | |
| All years*** | 2.7 | 1.6 | 1.0*** | 3.1 | 1.5*** | 92.9 |
| 1*** | 6.1 | 3.2 | 2.9*** | 7.6 | 4.2*** | 126.8 |
| 2*** | 2.8 | 2.1 | 0.8*** | 3.1 | 1.1*** | 55.4 |
| 3 | 1.3 | 1.1 | 0.2 | 1.4 | 0.2 | 22.0 |
| 4 | 0.5 | 0.5 | 0.0 | 0.5 | -0.1 | -13.5 |
| Percentage Ever Received Vocational Training*** | 69.5 | 23.7 | 45.8*** | 88.9 | 67.0*** | 306.3 |

TABLE C.5 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Percentage of Weeks Received Vocational Training | 11.8 | 3.4 | 8.4*** | 15.3 | 12.4*** | 427.9 |
| Average Hours per Week Received Vocational Training, by Year | | | | | | |
| All years | 2.9 | 0.7 | 2.1*** | 3.7 | 3.1*** | 580.5 |
| 1** | 6.7 | 0.8 | 6.0*** | 9.3 | 8.8*** | 1,536.7 |
| 2 | 2.5 | 0.5 | 1.9*** | 3.1 | 2.9*** | 962.5 |
| 3 | 1.2 | 0.6 | 0.6*** | 1.3 | 0.9*** | 210.6 |
| 4 | 0.9 | 1.0 | 0.0 | 0.7 | -0.1 | -7.1 |
| Degrees, Diplomas, and Certificates Ever Received | | | | | | |
| GED certificate or high school diploma[e] | 47.9 | 32.3 | 15.6*** | 54.3 | 22.9*** | 72.7 |
| GED certificate[e] | 42.7 | 25.7 | 17.0*** | 49.3 | 24.9*** | 102.1 |
| High school diploma[e] | 5.0 | 6.5 | -1.5** | 4.8 | -2.1** | -30.7 |
| Vocational, technical, or trade certificate | 35.8 | 14.9 | 20.9*** | 44.6 | 30.6*** | 220.0 |
| College degree (two-year or four-year) | 0.9 | 1.0 | -0.2 | 0.8 | -0.3 | -25.4 |
| Average Highest Grade Completed at the 48-Month Interview** | | | | | | |
| Less than 9 | 5.3 | 5.4 | -0.1 | 5.1 | -0.1 | -2.9 |
| 9 to 11 | 72.4 | 71.4 | 1.0 | 71.8 | 1.5 | 2.1 |
| 12 | 19.1 | 19.5 | -0.4 | 19.8 | -0.6 | -2.8 |
| Greater than 12 | 3.2 | 3.7 | -0.5 | 3.4 | -0.7 | -17.7 |
| Average Highest Grade Completed** | 10.6 | 10.6 | 0.0 | 10.6 | 0.0 | 0.3 |
| **Sample Size** | **2,489** | **1,593** | **4,082** | **1,717** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to an outcome indicate the significance level of the statistical test for differences in the impacts across the three subgroups defined by age and high school credential status.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the proportion of program group members who enrolled in Job Corps. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] Figures pertain to those who did not have a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE C.6

## IMPACTS ON EDUCATION AND TRAINING OUTCOMES FOR 18- TO 24-YEAR-OLDS WITH A HIGH SCHOOL CREDENTIAL AT RANDOM ASSIGNMENT

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Ever Enrolled in a Program During the 48 Months After Random Assignment*** | 88.9 | 58.2 | 30.7*** | 100.0 | 45.4*** | 82.9 |
| Percentage Enrolled in a Program, by Quarter After Random Assignment | | | | | | |
| 1*** | 70.0 | 16.5 | 53.5*** | 94.0 | 79.2*** | 534.6 |
| 2*** | 60.5 | 20.7 | 39.7*** | 79.6 | 58.8*** | 282.7 |
| 3*** | 52.2 | 22.3 | 30.0*** | 66.7 | 44.3*** | 198.3 |
| 4*** | 44.3 | 25.2 | 19.1*** | 54.8 | 28.2*** | 106.3 |
| 5*** | 38.9 | 22.7 | 16.3*** | 45.3 | 24.1*** | 113.5 |
| 6*** | 32.0 | 21.7 | 10.2*** | 34.3 | 15.1*** | 79.1 |
| 7*** | 26.3 | 20.9 | 5.4*** | 27.5 | 8.0*** | 41.2 |
| 8*** | 24.2 | 19.7 | 4.5*** | 25.6 | 6.6*** | 35.0 |
| 9** | 23.7 | 20.1 | 3.6** | 24.6 | 5.3** | 27.5 |
| 10 | 22.0 | 21.5 | 0.5 | 22.5 | 0.7 | 3.2 |
| 11 | 22.2 | 20.6 | 1.6 | 22.6 | 2.4 | 11.9 |
| 12 | 18.9 | 19.2 | -0.3 | 17.8 | -0.4 | -2.1 |
| 13 | 18.6 | 18.6 | 0.0 | 18.0 | 0.1 | 0.3 |
| 14 | 18.3 | 18.3 | 0.0 | 17.2 | 0.0 | 0.0 |
| 15 | 17.9 | 17.4 | 0.5 | 16.6 | 0.7 | 4.3 |
| 16 | 18.3 | 17.1 | 1.2 | 17.6 | 1.7 | 10.9 |
| Average Percentage of Weeks Ever in Education or Training*** | 0.2 | 0.2 | 0.1*** | 0.3 | 0.1*** | 88.2 |
| Average Hours per Week Ever in Education or Training*** | 7.7 | 3.3 | 4.4*** | 9.5 | 6.5*** | 218.1 |
| Type of Programs Other than Job Corps Ever Attended | | | | | | |
| Any program*** | 55.0 | 57.6 | -2.6 | 50.5 | -3.8 | -7.0 |
| Vocational, technical, or trade school | 34.9 | 37.8 | -2.9 | 30.2 | -4.4 | -12.6 |
| Two-year college | 21.2 | 23.6 | -2.4 | 20.5 | -3.6 | -14.8 |
| Four-year college | 7.7 | 8.1 | -0.4 | 6.9 | -0.6 | -7.5 |
| Other | 3.0 | 4.8 | -1.8** | 2.8 | -2.7** | -48.7 |
| Percentage Ever Took Academic Classes*** | 59.6 | 35.5 | 24.1*** | 68.8 | 35.6*** | 107.3 |
| Average Percentage of Weeks Ever in Academic Classes*** | 10.6 | 8.4 | 2.2** | 11.1 | 3.3** | 42.1 |
| Average Hours per Week in Academic Classes, by Year | | | | | | |
| All years*** | 2.1 | 1.5 | 0.6*** | 2.2 | 0.9*** | 68.9 |
| 1*** | 3.2 | 1.8 | 1.3*** | 3.8 | 1.9*** | 101.4 |
| 2*** | 2.8 | 2.2 | 0.7* | 2.6 | 1.0* | 62.2 |
| 3 | 1.9 | 1.8 | 0.1 | 1.8 | 0.2 | 11.9 |
| 4 | 0.6 | 0.6 | 0.0 | 0.5 | -0.1 | -10.4 |
| Percentage Ever Received Vocational Training*** | 75.4 | 38.5 | 36.9*** | 92.5 | 54.7*** | 144.5 |
| Average Percentage of Weeks Received Vocational Training | 14.7 | 7.1 | 7.6*** | 18.5 | 11.2*** | 154.1 |

TABLE C.6 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Hours per Week Received Vocational Training, by Year | | | | | | |
| All years | 4.1 | 1.7 | 2.4*** | 5.3 | 3.6*** | 214.2 |
| 1** | 9.3 | 1.7 | 7.6*** | 13.0 | 11.2*** | 625.4 |
| 2 | 4.1 | 2.2 | 1.9*** | 5.2 | 2.8*** | 122.6 |
| 3 | 1.9 | 1.8 | 0.1 | 2.1 | 0.2 | 8.0 |
| 4 | 1.2 | 0.9 | 0.3 | 1.0 | 0.4 | 63.3 |
| Degrees, Diplomas, and Certificates Ever Received | | | | | | |
| Vocational, technical, or trade certificate | 47.2 | 22.4 | 24.8*** | 58.3 | 36.7*** | 170.6 |
| College degree (two-year or four-year) | 3.8 | 4.0 | -0.2 | 3.6 | -0.3 | -7.5 |
| Average Highest Grade Completed at the 48-Month Interview** | | | | | | |
| Less than 9 | 1.3 | 0.9 | 0.4** | 1.4 | 0.6** | 85.3 |
| 9 to 11 | 0.6 | 14.3 | -3.7 | 11.5 | -5.5 | -32.4 |
| 12 | 68.0 | 63.9 | 4.2 | 69.2 | 6.2 | 9.8 |
| Greater than 12 | 20.0 | 20.9 | -0.9 | 17.9 | -1.3 | -6.7 |
| Average Highest Grade Completed** | 12.1 | 12.0 | 0.1 | 12.0 | 0.1 | 0.7 |
| **Sample Size** | **1,559** | **965** | **2,524** | **1,049** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to an outcome indicate the significance level of the statistical test for differences in the impacts across the three subgroups defined by age and high school credential status.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the proportion of program group members who enrolled in Job Corps. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE C.7

## IMPACTS ON KEY EDUCATION AND TRAINING OUTCOMES, BY GENDER, RESIDENTIAL DESIGNATION STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE

| | Percentage Ever Participated in Education or Training | | Average Hours per Week in Education and Training | | Average Hours per Week in Academic Classes | | Average Hours per Week in Vocational Training | | Percentage Received a GED[a] | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Gender | | | | | | | | | | |
| Males | 68.9 | 31.3*** | 3.9 | 4.7*** | 2.5 | 0.5*** | 0.8 | 3.0*** | 27.8 | 18.2*** |
| Females | 75.8 | 25.2*** | 4.5 | 5.0*** | 2.4 | 1.2*** | 1.1 | 3.1*** | 24.6 | 25.6*** |
| (P-value)[c] | | 0.000*** | | 0.685 | | .042** | | 0.659 | | .067* |
| Residential Designees | | | | | | | | | | |
| Males | 69.0 | 31.3*** | 3.9 | 4.7*** | 2.6 | 0.5*** | 0.8 | 3.0*** | 28.2 | 18.0*** |
| Females | 75.7 | 25.3*** | 4.6 | 4.7*** | 2.5 | 1.2*** | 1.1 | 3.2*** | 24.2 | 26.3*** |
| (P-value)[c] | | 0.000*** | | 0.532 | | 0.068* | | 0.863 | | 0.038** |
| Nonresidential Designees | | | | | | | | | | |
| Males | 67.6 | 30.2*** | 3.8 | 4.6*** | 2.4 | 0.5 | 1.1 | 2.5*** | 21.1 | 21.0*** |
| Females | 76.1 | 24.5*** | 4.1 | 5.8*** | 2.1 | 1.3*** | 1.3 | 3.1*** | 26.0 | 22.5*** |
| (P-value)[c] | | 0.114 | | 0.535 | | 0.390 | | 0.790 | | 0.882 |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 71.6 | 28.9*** | 4.3 | 4.9*** | 2.5 | 0.9*** | 1.0 | 3.1*** | 25.2 | 22.1*** |
| Ever arrested for nonserious crimes | 72.8 | 28.4*** | 3.9 | 4.2*** | 2.5 | 0.3 | 0.9 | 2.8*** | 30.4 | 17.6*** |
| Ever arrested for serious crimes | 70.6 | 33.4*** | 3.8 | 4.5*** | 2.4 | 0.6 | 0.4 | 3.6*** | 34.8 | 9.3 |
| (P-value)[c] | | 0.541 | | 0.064* | | 0.149 | | 0.211 | | 0.069* |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 67.5 | 34.4*** | 3.3 | 4.7*** | 2.1 | 0.0 | 0.8 | 3.4*** | 31.1 | 27.2*** |
| Black non-Hispanic | 73.1 | 27.4*** | 4.4 | 4.6*** | 2.6 | 1.0*** | 1.0 | 2.7*** | 25.2 | 17.8*** |
| Hispanic | 73.0 | 27.4*** | 4.2 | 5.8*** | 2.5 | 1.5*** | 0.9 | 3.5*** | 25.0 | 23.0*** |
| Other[d] | 75.2 | 23.9*** | 5.1 | 4.5*** | 3.0 | 0.8 | 1.3 | 3.1*** | 24.0 | 14.5*** |
| (P-value)[c] | | 0.058* | | 0.038** | | 0.002*** | | 0.085* | | 0.073* |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 72.3 | 27.4*** | 4.1 | 4.9*** | 2.5 | 0.6 | 0.9 | 2.6*** | 27.6 | 19.9*** |
| On or after 3/1/95 (after ZT) | 71.6 | 29.4*** | 4.1 | 4.8*** | 2.5 | 0.8*** | 0.9 | 3.1*** | 26.3 | 21.2*** |
| (P-value)[c] | | 0.223 | | 0.944 | | 0.623 | | 0.138 | | 0.582 |

TABLE C.7 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and SPAMIS data, for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Only includes sample members who did not have a GED or high school diploma at baseline.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[d] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# APPENDIX D

# SUPPLEMENTARY TABLES TO CHAPTER VI

## TABLE D.1

## IMPACTS ON THE PERCENTAGE OF WEEKS EMPLOYED OR IN AN EDUCATION PROGRAM

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Percentage of Weeks in Any Activity, by Quarter After Random Assignment | | | | | | |
| 1 | 67.8 | 45.0 | 22.7*** | 75.9 | 31.6*** | 71.3 |
| 2 | 71.8 | 53.8 | 17.9*** | 79.1 | 24.9*** | 46.1 |
| 3 | 68.6 | 58.1 | 10.5*** | 73.1 | 14.6*** | 25.0 |
| 4 | 65.0 | 59.9 | 5.0*** | 67.3 | 7.0*** | 11.6 |
| 5 | 62.3 | 58.9 | 3.4*** | 63.6 | 4.7*** | 7.9 |
| 6 | 60.4 | 57.7 | 2.7*** | 60.9 | 3.7*** | 6.5 |
| 7 | 59.8 | 58.2 | 1.6* | 60.1 | 2.2* | 3.8 |
| 8 | 60.9 | 58.6 | 2.3*** | 61.1 | 3.2*** | 5.5 |
| 9 | 62.7 | 60.5 | 2.1** | 63.0 | 3.0** | 5.0 |
| 10 | 63.8 | 61.3 | 2.5*** | 64.5 | 3.5*** | 5.7 |
| 11 | 64.8 | 61.9 | 2.9*** | 65.1 | 4.0*** | 6.5 |
| 12 | 64.4 | 62.2 | 2.2*** | 64.6 | 3.1*** | 5.0 |
| 13 | 65.2 | 62.6 | 2.6*** | 65.2 | 3.6*** | 5.9 |
| 14 | 65.4 | 63.4 | 1.9** | 65.5 | 2.7** | 4.2 |
| 15 | 66.3 | 64.1 | 2.2*** | 66.6 | 3.0*** | 4.8 |
| 16 | 67.3 | 65.1 | 2.2*** | 67.5 | 3.0*** | 4.7 |
| Percentage of Weeks in Any Activity | 64.7 | 59.2 | 5.5*** | 66.1 | 7.6*** | 13.1 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.2

## IMPACTS ON HOURS PER WEEK EMPLOYED OR IN AN EDUCATION PROGRAM

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Hours per Week in Any Activity, by Quarter After Random Assignment | | | | | | |
| 1 | 28.8 | 17.4 | 11.4*** | 32.5 | 15.8*** | 94.7 |
| 2 | 30.5 | 21.6 | 9.0*** | 33.8 | 12.5*** | 58.4 |
| 3 | 29.3 | 23.5 | 5.8*** | 31.3 | 8.1*** | 35.1 |
| 4 | 27.7 | 24.1 | 3.5*** | 28.7 | 4.9*** | 20.5 |
| 5 | 26.7 | 24.4 | 2.3*** | 27.4 | 3.2*** | 13.2 |
| 6 | 26.6 | 24.8 | 1.7*** | 27.0 | 2.4*** | 9.7 |
| 7 | 26.7 | 25.2 | 1.5*** | 27.1 | 2.0*** | 8.2 |
| 8 | 27.4 | 25.8 | 1.6*** | 27.7 | 2.2*** | 8.7 |
| 9 | 28.0 | 26.7 | 1.4*** | 28.4 | 1.9*** | 7.2 |
| 10 | 28.3 | 26.7 | 1.6*** | 29.0 | 2.2*** | 8.2 |
| 11 | 29.1 | 27.3 | 1.8*** | 29.5 | 2.5*** | 9.1 |
| 12 | 29.2 | 27.8 | 1.4*** | 29.6 | 1.9*** | 6.9 |
| 13 | 29.7 | 28.5 | 1.3*** | 30.0 | 1.8*** | 6.3 |
| 14 | 29.9 | 28.7 | 1.2** | 30.1 | 1.6** | 5.6 |
| 15 | 30.2 | 28.9 | 1.3*** | 30.3 | 1.8*** | 6.4 |
| 16 | 30.3 | 28.9 | 1.4*** | 30.4 | 1.9*** | 6.6 |
| Average Hours per Week in Any Activity | 28.3 | 25.2 | 3.1*** | 29.1 | 4.4*** | 17.7 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE D.3

IMPACTS ON EMPLOYMENT AND EARNINGS FOR 16- AND 17-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1* | 27.4 | 34.2 | -6.8*** | 24.4 | -8.7*** | -26.4 |
| 2** | 26.4 | 38.5 | -12.1*** | 22.2 | -15.6*** | -41.3 |
| 3 | 35.7 | 45.4 | -9.7*** | 33.1 | -12.6*** | -27.5 |
| 4 | 44.0 | 51.1 | -7.1*** | 42.1 | -9.2*** | -17.9 |
| 5 | 46.1 | 48.1 | -2.0 | 45.8 | -2.6 | -5.4 |
| 6 | 45.1 | 45.3 | -0.2 | 45.0 | -0.3 | -0.6 |
| 7 | 49.5 | 48.0 | 1.5 | 49.4 | 1.9 | 4.0 |
| 8** | 53.1 | 51.0 | 2.1 | 53.6 | 2.8 | 5.4 |
| 9* | 57.5 | 55.6 | 1.9 | 58.6 | 2.5 | 4.5 |
| 10 | 60.9 | 58.1 | 2.8** | 61.7 | 3.7** | 6.3 |
| 11 | 62.0 | 59.1 | 2.9** | 63.0 | 3.7** | 6.2 |
| 12 | 61.0 | 57.8 | 3.2** | 61.0 | 4.1** | 7.2 |
| 13 | 62.2 | 58.9 | 3.3** | 62.3 | 4.2** | 7.3 |
| 14 | 63.9 | 61.6 | 2.3 | 64.3 | 2.9 | 4.8 |
| 15 | 65.8 | 62.9 | 2.9** | 66.9 | 3.8** | 6.0 |
| 16 | 67.2 | 65.4 | 1.9 | 68.1 | 2.4 | 3.6 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1*** | 22.0 | 29.4 | -7.4*** | 19.5 | -9.6*** | -33.0 |
| 2** | 37.8 | 38.1 | -0.3 | 37.7 | -0.4 | -1.1 |
| 3*** | 48.7 | 46.4 | 2.3** | 49.2 | 2.9** | 6.4 |
| 4 | 55.1 | 52.4 | 2.7** | 55.6 | 3.4** | 6.6 |
| Average Hours per Week Employed, by Quarter | | | | | | |
| 1*** | 9.0 | 11.3 | -2.4*** | 7.9 | -3.0*** | -27.8 |
| 2*** | 16.7 | 16.2 | 0.5 | 16.6 | 0.6 | 3.7 |
| 3** | 22.1 | 20.4 | 1.7*** | 22.5 | 2.2*** | 10.8 |
| 4** | 25.2 | 24.0 | 1.2** | 25.7 | 1.6** | 6.5 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1*** | 29.4 | 41.1 | -11.7*** | 22.6 | -15.0*** | -40.0 |
| 2*** | 42.1 | 57.7 | -15.6*** | 33.2 | -20.1*** | -37.7 |
| 3*** | 59.6 | 68.0 | -8.4** | 52.6 | -10.8** | -17.1 |
| 4 | 69.4 | 77.1 | -7.7** | 65.3 | -9.9** | -13.2 |
| 5** | 83.1 | 84.0 | -0.8 | 81.0 | -1.1 | -1.3 |
| 6 | 100.8 | 96.7 | 4.1 | 99.0 | 5.2 | 5.6 |
| 7** | 119.5 | 104.8 | 14.7*** | 118.4 | 18.9*** | 19.0 |
| 8 | 129.4 | 118.9 | 10.6** | 129.4 | 13.6** | 11.8 |
| 9 | 141.0 | 128.0 | 13.0** | 141.5 | 16.8** | 13.4 |
| 10 | 148.0 | 132.1 | 15.9*** | 149.7 | 20.5*** | 15.8 |
| 11 | 159.1 | 138.6 | 20.5*** | 162.9 | 26.4*** | 19.4 |
| 12* | 170.2 | 150.7 | 19.5*** | 173.7 | 25.1*** | 16.9 |
| 13*** | 180.7 | 162.8 | 18.0*** | 183.4 | 23.2*** | 14.5 |
| 14** | 188.6 | 173.6 | 15.0** | 191.8 | 19.3** | 11.2 |
| 15** | 192.8 | 180.0 | 12.8** | 198.8 | 16.5** | 9.0 |
| 16** | 194.2 | 180.3 | 14.0** | 199.0 | 18.0** | 10.0 |

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TABLE D.3 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 50.2 | 60.7 | -10.5*** | 43.9 | -13.5*** | -23.6 |
| 2* | 107.2 | 101.6 | 5.6 | 105.5 | 7.3 | 7.4 |
| 3* | 153.6 | 136.4 | 17.1*** | 155.5 | 22.1*** | 16.6 |
| 4** | 188.1 | 174.8 | 13.3** | 191.9 | 17.2** | 9.8 |
| Average Total Earnings per Week (in 1995 Dollars)*** | 121.7 | 115.1 | 6.7** | 120.9 | 8.6** | 7.7 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.28 | 7.04 | 0.24* | 7.24 | 0.31* | 4.5 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 52.8 | 51.1 | 1.7 | 53.9 | 2.2 | 4.2 |
| Paid sick leave | 41.8 | 41.2 | 0.6 | 43.2 | 0.8 | 1.8 |
| Paid vacation | 58.5 | 57.5 | 1.0 | 58.7 | 1.2 | 2.1 |
| Retirement or pension benefits | 42.1 | 39.9 | 2.1 | 43.8 | 2.7 | 6.7 |
| **Sample Size** | **2,742** | **1,907** | **4,649** | **2,132** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three age groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.4

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR 18- AND 19-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1* | 35.9 | 45.2 | -9.2*** | 30.1 | -13.4*** | -30.7 |
| 2** | 36.6 | 51.5 | -14.8*** | 29.3 | -21.5*** | -42.3 |
| 3 | 44.5 | 56.2 | -11.6*** | 39.5 | -16.8*** | -29.8 |
| 4 | 52.7 | 61.2 | -8.5*** | 48.5 | -12.3*** | -20.2 |
| 5 | 55.7 | 61.2 | -5.5*** | 53.0 | -8.0*** | -13.1 |
| 6 | 54.7 | 58.9 | -4.2** | 53.3 | -6.0** | -10.1 |
| 7 | 56.6 | 59.8 | -3.2* | 55.5 | -4.6* | -7.6 |
| 8** | 60.0 | 62.3 | -2.3 | 60.0 | -3.3 | -5.3 |
| 9* | 62.8 | 64.5 | -1.7 | 63.3 | -2.5 | -3.7 |
| 10 | 66.4 | 66.9 | -0.5 | 68.3 | -0.8 | -1.1 |
| 11 | 67.8 | 66.7 | 1.1 | 68.3 | 1.6 | 2.3 |
| 12 | 66.3 | 65.0 | 1.2 | 66.8 | 1.8 | 2.7 |
| 13 | 67.6 | 65.5 | 2.1 | 69.6 | 3.0 | 4.5 |
| 14 | 66.9 | 66.9 | 0.1 | 68.0 | 0.1 | 0.1 |
| 15 | 69.6 | 66.7 | 2.8* | 70.8 | 4.1 | 6.2 |
| 16 | 71.9 | 70.5 | 1.4 | 73.3 | 2.1* | 2.9 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1*** | 29.9 | 41.0 | -11.2*** | 24.6 | -16.2*** | -39.7 |
| 2** | 46.4 | 51.2 | -4.8*** | 44.7 | -6.9*** | -13.4 |
| 3*** | 55.5 | 57.0 | -1.5 | 56.3 | -2.1 | -3.6 |
| 4 | 60.7 | 59.1 | 1.5 | 61.7 | 2.2 | 3.7 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1*** | 12.4 | 17.2 | -4.9*** | 10.2 | -7.1*** | -40.8 |
| 2*** | 20.3 | 23.0 | -2.7*** | 19.6 | -3.9*** | -16.6 |
| 3** | 24.9 | 25.3 | -0.4 | 25.5 | -0.6 | -2.5 |
| 4** | 27.5 | 27.3 | 0.2 | 28.1 | 0.3 | 1.1 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1*** | 50.1 | 74.8 | -24.7*** | 37.1 | -35.8*** | -49.1 |
| 2*** | 62.6 | 96.7 | -34.1*** | 48.4 | -49.3*** | -50.5 |
| 3*** | 83.0 | 110.7 | -27.8*** | 71.4 | -40.2*** | -36.0 |
| 4 | 99.4 | 116.2 | -16.8*** | 89.8 | -24.3*** | -21.3 |
| 5** | 115.4 | 133.2 | -17.7*** | 108.2 | -25.7*** | -19.2 |
| 6 | 132.3 | 143.7 | -11.3* | 130.0 | -16.4* | -11.2 |
| 7** | 145.6 | 154.3 | -8.7 | 144.3 | -12.6 | -8.0 |
| 8 | 155.5 | 159.6 | -4.1 | 155.6 | -5.9 | -3.7 |
| 9 | 165.6 | 167.5 | -2.0 | 169.2 | -2.8 | -1.6 |
| 10 | 173.4 | 172.5 | 0.9 | 181.7 | 1.2 | 0.7 |
| 11 | 188.7 | 182.4 | 6.2 | 193.6 | 9.0 | 4.9 |
| 12* | 197.1 | 192.0 | 5.1 | 203.1 | 7.4 | 3.8 |
| 13*** | 202.9 | 203.3 | -0.4 | 210.0 | -0.7 | -0.3 |
| 14** | 205.8 | 205.0 | 0.9 | 210.1 | 1.3 | 0.6 |
| 15** | 211.9 | 205.5 | 6.5 | 213.4 | 9.3 | 4.6 |
| 16** | 216.3 | 209.5 | 6.8 | 217.8 | 9.8 | 4.7 |

TABLE D.4 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 73.3 | 100.0 | -26.7*** | 62.0 | -38.6*** | -38.4 |
| 2* | 136.9 | 146.8 | -10.0* | 133.6 | -14.4* | -9.7 |
| 3* | 179.6 | 177.2 | 2.4 | 185.0 | 3.5 | 1.9 |
| 4** | 210.4 | 206.5 | 3.9 | 213.1 | 5.6 | 2.7 |
| Average Total Earnings, per Week (in 1995 Dollars)*** | 142.9 | 152.1 | -9.2** | 142.2 | -13.4** | -8.6 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.52 | 7.32 | 0.20 | 7.44 | 0.28 | 4.0 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 58.4 | 54.6 | 3.8* | 59.4 | 5.5* | 10.2 |
| Paid sick leave | 50.4 | 46.5 | 3.9* | 51 | 5.7* | 12.6 |
| Paid vacation | 63.5 | 62.7 | 0.7 | 63.7 | 1.1 | 1.7 |
| Retirement or pension benefits | 49.6 | 45.1 | 4.5** | 50.7 | 6.6** | 14.9 |
| **Sample Size** | **2,175** | **1,402** | **3,577** | **1,518** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three age groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.5

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR 20- TO 24-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1* | 38.7 | 50.9 | -12.1*** | 32.4 | -18.2*** | -35.9 |
| 2** | 37.8 | 56.6 | -18.7*** | 27.9 | -28.1*** | -50.2 |
| 3* | 48.0 | 60.9 | -12.9*** | 39.5 | -19.3*** | -32.8 |
| 4 | 55.4 | 63.8 | -8.4*** | 50.9 | -12.6*** | -19.8 |
| 5 | 58.8 | 64.5 | -5.7*** | 56.7 | -8.6*** | -13.1 |
| 6 | 59.7 | 62.8 | -3.1* | 59.0 | -4.7* | -7.4 |
| 7 | 62.0 | 63.1 | -1.0 | 62.2 | -1.6 | -2.5 |
| 8** | 66.8 | 63.2 | 3.6** | 67.3 | 5.4** | 8.7 |
| 9* | 70.3 | 66.9 | 3.4** | 71.8 | 5.1** | 7.6 |
| 10 | 71.9 | 68.5 | 3.4** | 73.0 | 5.1** | 7.5 |
| 11 | 74.2 | 69.4 | 4.8*** | 75.4 | 7.2*** | 10.6 |
| 12 | 73.9 | 68.6 | 5.2*** | 75.1 | 7.9*** | 11.7 |
| 13 | 72.9 | 67.8 | 5.1*** | 73.6 | 7.7*** | 11.7 |
| 14 | 73.5 | 68.4 | 5.0*** | 74.0 | 7.6*** | 11.4 |
| 15 | 74.0 | 68.5 | 5.5*** | 75.1 | 8.2*** | 12.3 |
| 16 | 75.8 | 71.6 | 4.3*** | 75.5 | 6.4*** | 9.2 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1*** | 33.4 | 46.9 | -13.5*** | 26.4 | -20.3*** | -43.4 |
| 2** | 53.0 | 54.8 | -1.8 | 51.8 | -2.8 | -5.1 |
| 3*** | 64.7 | 60.2 | 4.4*** | 65.8 | 6.7*** | 11.3 |
| 4 | 67.3 | 62.3 | 5.0*** | 68.2 | 7.5*** | 12.4 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1*** | 13.9 | 20.0 | -6.1*** | 10.8 | -9.2*** | -46.0 |
| 2*** | 23.4 | 24.2 | -0.8 | 23.1 | -1.2 | -4.9 |
| 3** | 28.4 | 26.7 | 1.7** | 29.0 | 2.5** | 9.6 |
| 4** | 30.4 | 27.4 | 3.0*** | 30.7 | 4.5*** | 17.4 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1*** | 57.1 | 92.7 | -35.6*** | 37.7 | -53.3*** | -58.6 |
| 2*** | 76.1 | 122.7 | -46.6*** | 47.8 | -69.8*** | -59.3 |
| 3*** | 98.4 | 134.2 | -35.8*** | 72.6 | -53.6*** | -42.5 |
| 4 | 119.0 | 139.0 | -20.0*** | 100.8 | -30.0*** | -22.9 |
| 5** | 139.8 | 151.1 | -11.3* | 131.6 | -16.9* | -11.4 |
| 6 | 159.7 | 162.5 | -2.8 | 154.7 | -4.3 | -2.7 |
| 7** | 172.8 | 170.7 | 2.1 | 171.0 | 3.2 | 1.9 |
| 8 | 186.8 | 175.3 | 11.4 | 186.3 | 17.1 | 10.1 |
| 9 | 199.9 | 184.8 | 15.0** | 201.4 | 22.5** | 12.6 |
| 10 | 205.3 | 188.0 | 17.4** | 209.9 | 26.0** | 14.1 |
| 11 | 223.8 | 204.3 | 19.5*** | 226.3 | 29.2*** | 14.8 |
| 12* | 234.2 | 205.7 | 28.5*** | 236.2 | 42.7*** | 22.1 |
| 13*** | 245.2 | 208.7 | 36.5*** | 250.6 | 54.6*** | 27.9 |
| 14** | 246.5 | 213.1 | 33.4*** | 251.6 | 50.1*** | 24.8 |
| 15** | 247.6 | 214.2 | 33.3*** | 250.0 | 49.9*** | 25.0 |
| 16** | 254.1 | 216.8 | 37.3*** | 253.4 | 55.8*** | 28.3 |

TABLE D.5 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 87.3 | 122.7 | -35.4*** | 65.9 | -53.1*** | -44.6 |
| 2* | 164.4 | 166.0 | -1.6 | 160.8 | -2.3 | -1.4 |
| 3* | 215.5 | 195.3 | 20.1*** | 218.4 | 30.1*** | 16.0 |
| 4** | 247.7 | 214.1 | 33.5*** | 250.2 | 50.2*** | 25.1 |
| Average Total Earnings per Week (in 1995 Dollars)*** | 176.9 | 169.6 | 7.3 | 172.9 | 10.9 | 6.7 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.95 | 7.76 | 0.19 | 7.98 | 0.28 | 3.7 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 62.2 | 58.4 | 3.8* | 63.6 | 5.7* | 9.9 |
| Paid sick leave | 51.1 | 46.9 | 4.2* | 51.6 | 6.4* | 14.0 |
| Paid vacation | 68.1 | 62.8 | 5.3** | 67.9 | 8.0** | 13.3 |
| Retirement or pension benefits | 54.9 | 47.1 | 7.9*** | 57.8 | 11.8*** | 25.7 |
| **Sample Size** | **1,911** | **1,176** | **3,087** | **1,275** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three age groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.6

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR MALES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 34.5 | 43.3 | -8.7*** | 29.7 | -11.7*** | -28.3 |
| 2** | 32.8 | 49.2 | -16.5*** | 25.9 | -22.1*** | -46.0 |
| 3* | 42.4 | 55.0 | -12.6*** | 36.9 | -16.9*** | -31.4 |
| 4 | 51.5 | 59.7 | -8.2*** | 47.4 | -11*** | -18.9 |
| 5 | 55.6 | 58.6 | -3.0** | 53.5 | -4.0** | -6.9 |
| 6 | 54.8 | 56.3 | -1.5 | 53.5 | -2.0 | -3.6 |
| 7 | 57.7 | 58.3 | -0.6 | 56.4 | -0.8 | -1.3 |
| 8 | 61.1 | 60.5 | 0.6 | 60.3 | 0.8 | 1.3 |
| 9 | 65.1 | 64.8 | 0.2 | 65.0 | 0.3 | 0.5 |
| 10 | 68.9 | 67.1 | 1.8 | 69.3 | 2.4 | 3.5 |
| 11 | 70.1 | 67.9 | 2.2* | 70.6 | 2.9* | 4.3 |
| 12 | 68.6 | 66.0 | 2.6** | 69.1 | 3.5** | 5.4 |
| 13 | 69.2 | 66.0 | 3.1*** | 69.5 | 4.2*** | 6.4 |
| 14 | 70.0 | 68.4 | 1.7 | 70.3 | 2.2 | 3.3 |
| 15 | 71.9 | 68.8 | 3.1*** | 72.8 | 4.1*** | 6.0 |
| 16 | 72.8 | 70.9 | 1.9* | 73.8 | 2.6* | 3.6 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1* | 28.2 | 39.3 | -11.1*** | 23.4 | -14.9*** | -38.9 |
| 2 | 47.3 | 49.0 | -1.7* | 45.6 | -2.3* | -4.8 |
| 3 | 58.1 | 57.0 | 1.1 | 58.1 | 1.4 | 2.6 |
| 4 | 63.1 | 60.5 | 2.6*** | 63.5 | 3.5*** | 5.8 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1*** | 12.1 | 16.9 | -4.8*** | 10.0 | -6.5*** | -39.3 |
| 2 | 21.7 | 22.4 | -0.7 | 20.8 | -0.9 | -4.4 |
| 3 | 27.1 | 26.3 | 0.8* | 27.2 | 1.1* | 4.2 |
| 4 | 29.9 | 28.7 | 1.2** | 30.1 | 1.6** | 5.6 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1* | 48.9 | 73.9 | -25.0*** | 34.1 | -33.6*** | -49.6 |
| 2*** | 63.6 | 99.5 | -35.9*** | 45.8 | -48.2*** | -51.2 |
| 3** | 85.8 | 112.8 | -27.0*** | 70.2 | -36.2*** | -34.0 |
| 4*** | 103.4 | 122.7 | -19.2*** | 90.4 | -25.8*** | -22.2 |
| 5 | 123.5 | 133.9 | -10.4** | 114.1 | -14.0** | -10.9 |
| 6 | 144.7 | 145.4 | -0.8 | 137.9 | -1.1 | -0.8 |
| 7 | 161.6 | 156.9 | 4.7 | 155.4 | 6.3 | 4.2 |
| 8 | 175.3 | 164.4 | 10.9** | 169.8 | 14.7** | 9.5 |
| 9 | 187.4 | 177.8 | 9.6* | 183.3 | 12.9* | 7.5 |
| 10 | 197.3 | 184.4 | 12.9** | 196.8 | 17.3** | 9.6 |
| 11 | 211.7 | 195.7 | 16.0*** | 211.1 | 21.5*** | 11.3 |
| 12 | 223.6 | 203.0 | 20.6*** | 223.4 | 27.7*** | 14.1 |
| 13 | 233.1 | 210.8 | 22.2*** | 234.3 | 29.8*** | 14.6 |
| 14 | 239.2 | 220.7 | 18.5*** | 240.1 | 24.8*** | 11.5 |
| 15 | 243.0 | 225.4 | 17.6*** | 245.0 | 23.7*** | 10.7 |
| 16 | 246.4 | 225.5 | 20.8*** | 246.9 | 28.0*** | 12.8 |

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TABLE D.6 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year 1 (in 1995 Dollars) | | | | | | |
| 1*** | 75.1 | 102.3 | -27.3*** | 61.0 | -36.6*** | -37.5 |
| 2 | 150.7 | 150.6 | 0.1 | 143.3 | 0.1 | 0.1 |
| 3 | 204.4 | 188.9 | 15.4*** | 203.0 | 20.7*** | 11.4 |
| 4 | 239.8 | 222.1 | 17.7*** | 240.3 | 23.8*** | 11.0 |
| Average Total Earnings per Week (in 1995 Dollars) | 163.8 | 160.8 | 3.0 | 158.8 | 4.0 | 2.6 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.87 | 7.62 | 0.25** | 7.78 | 0.34** | 4.5 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 58.8 | 56.1 | 2.7* | 59.5 | 3.6* | 6.5 |
| Paid sick leave | 47.6 | 45.7 | 1.9 | 47.8 | 2.6 | 5.7 |
| Paid vacation** | 62.9 | 62.8 | 0.2 | 62.6 | 0.2 | 0.3 |
| Retirement or pension benefits | 50.1 | 46.3 | 3.8** | 50.9 | 5.0** | 11.0 |
| **Sample Size** | **3,741** | **2,787** | **6,528** | **2,799** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two gender groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.7

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 31.2 | 40.4 | -9.2*** | 25.7 | -13.4*** | -34.3 |
| 2** | 32.7 | 44.9 | -12.2*** | 25.5 | -17.8*** | -41.1 |
| 3* | 41.1 | 50.0 | -9.0*** | 36.1 | -13.1*** | -26.6 |
| 4 | 47.5 | 54.8 | -7.3*** | 44.4 | -10.7*** | -19.4 |
| 5 | 48.3 | 53.9 | -5.7*** | 46.5 | -8.3*** | -15.1 |
| 6 | 48.3 | 51.5 | -3.1** | 47.3 | -4.6** | -8.9 |
| 7 | 51.6 | 52.2 | -0.6 | 51.4 | -0.9 | -1.7 |
| 8 | 56.1 | 54.0 | 2.1 | 57.0 | 3.0 | 5.6 |
| 9 | 59.2 | 56.5 | 2.7* | 60.6 | 4.0* | 7.0 |
| 10 | 61.0 | 58.7 | 2.3 | 62.3 | 3.3 | 5.7 |
| 11 | 62.9 | 59.0 | 3.9*** | 63.2 | 5.7*** | 9.9 |
| 12 | 62.7 | 58.7 | 4.0*** | 62.0 | 5.9*** | 10.5 |
| 13 | 63.4 | 59.5 | 4.0*** | 64.0 | 5.8*** | 9.9 |
| 14 | 63.8 | 60.3 | 3.5** | 64.0 | 5.1** | 8.6 |
| 15 | 65.5 | 61.0 | 4.5*** | 66.0 | 6.6*** | 11.1 |
| 16 | 68.5 | 65.4 | 3.1** | 68.1 | 4.6** | 7.2 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1* | 26.7 | 35.5 | -8.8*** | 21.7 | -12.9*** | -37.4 |
| 2 | 40.9 | 43.5 | -2.6** | 39.9 | -3.8** | -8.7 |
| 3 | 51.0 | 48.3 | 2.7** | 51.4 | 4.0** | 8.4 |
| 4 | 56.0 | 52.4 | 3.7*** | 56.1 | 5.4*** | 10.6 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1*** | 10.3 | 13.5 | -3.2*** | 8.3 | -4.6*** | -35.8 |
| 2 | 16.8 | 17.9 | -1.0* | 16.6 | -1.5* | -8.5 |
| 3 | 21.2 | 19.8 | 1.4*** | 21.7 | 2.1*** | 10.8 |
| 4 | 23.7 | 21.9 | 1.8*** | 23.8 | 2.6*** | 12.4 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1* | 35.9 | 53.5 | -17.6*** | 25.3 | -25.8*** | -50.4 |
| 2*** | 49.9 | 70.0 | -20.1*** | 34.6 | -29.4*** | -46.0 |
| 3** | 66.1 | 79.8 | -13.7*** | 52.6 | -20.1*** | -27.6 |
| 4*** | 76.7 | 82.2 | -5.5 | 67.8 | -8.0 | -10.5 |
| 5 | 87.9 | 93.9 | -6.1 | 83.1 | -8.9 | -9.6 |
| 6 | 101.4 | 105.8 | -4.4 | 98.3 | -6.4 | -6.1 |
| 7 | 114.9 | 111.0 | 3.9 | 114.8 | 5.7 | 5.2 |
| 8 | 122.0 | 121.6 | 0.4 | 123.4 | 0.6 | 0.5 |
| 9 | 132.5 | 123.7 | 8.8* | 136.2 | 12.9* | 10.4 |
| 10 | 134.7 | 124.3 | 10.4** | 139.4 | 15.2** | 12.3 |
| 11 | 149.2 | 132.8 | 16.4*** | 152.2 | 23.9*** | 18.7 |
| 12 | 157.0 | 142.7 | 14.3*** | 159.4 | 20.9*** | 15.1 |
| 13 | 165.6 | 154.3 | 11.3** | 168.1 | 16.5** | 10.9 |
| 14 | 168.0 | 155.3 | 12.6** | 169.2 | 18.5** | 12.3 |
| 15 | 171.9 | 156.0 | 15.9*** | 170.7 | 23.3*** | 15.8 |
| 16 | 176.0 | 161.1 | 14.8*** | 174.0 | 21.7*** | 14.3 |

TABLE D.7 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 56.9 | 71.4 | -14.5*** | 45.2 | -21.2*** | -31.9 |
| 2 | 105.8 | 108.0 | -2.3 | 103.9 | -3.3 | -3.1 |
| 3 | 141.5 | 130.2 | 11.3*** | 144.4 | 16.5*** | 12.9 |
| 4 | 170.6 | 156.5 | 14.1*** | 170.5 | 20.6*** | 13.8 |
| Average Total Earnings per Week (in 1995 Dollars) | 114.3 | 113.2 | 1.1 | 111.8 | 1.6 | 1.5 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.06 | 6.88 | 0.18* | 7.02 | 0.27* | 4.0 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 55.1 | 51.6 | 3.5* | 55.7 | 5.2* | 10.2 |
| Paid sick leave | 46.7 | 42.6 | 4.2** | 48.0 | 6.1** | 14.5 |
| Paid vacation** | 62.8 | 57.4 | 5.4*** | 62.7 | 7.9*** | 14.3 |
| Retirement or pension benefits | 45.4 | 39.3 | 6.1*** | 47.5 | 9.0*** | 23.3 |
| **Sample Size** | **3,087** | **1,698** | **4,785** | **2,126** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two gender groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.8

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR MALE RESIDENTIAL DESIGNEES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 34.0 | 42.9 | -8.9*** | 29.0 | -11.9*** | -29.0 |
| 2** | 31.9 | 49.0 | -17.1*** | 24.9 | -22.8*** | -47.8 |
| 3* | 41.7 | 55.0 | -13.4*** | 36.2 | -17.8*** | -33.0 |
| 4 | 50.8 | 59.5 | -8.7*** | 46.7 | -11.6*** | -19.9 |
| 5 | 55.3 | 58.6 | -3.3** | 53.1 | -4.4** | -7.7 |
| 6 | 54.4 | 56.2 | -1.8 | 52.9 | -2.4 | -4.4 |
| 7 | 57.3 | 58.1 | -0.9 | 55.7 | -1.1 | -2.0 |
| 8 | 60.6 | 60.5 | 0.1 | 59.6 | 0.2 | 0.3 |
| 9 | 64.7 | 64.9 | -0.1 | 64.3 | -0.2 | -0.3 |
| 10 | 68.9 | 67.3 | 1.6 | 69.0 | 2.1 | 3.1 |
| 11 | 69.8 | 67.9 | 1.9 | 70.1 | 2.6 | 3.8 |
| 12 | 68.4 | 65.8 | 2.6** | 68.7 | 3.4** | 5.3 |
| 13 | 69.1 | 65.9 | 3.1*** | 69.3 | 4.2*** | 6.4 |
| 14 | 69.9 | 68.2 | 1.7 | 70.0 | 2.2 | 3.3 |
| 15 | 71.7 | 68.9 | 2.8** | 72.6 | 3.8** | 5.5 |
| 16 | 72.7 | 70.9 | 1.9 | 73.7 | 2.5 | 3.5 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1** | 27.5 | 38.9 | -11.4*** | 22.7 | -15.2*** | -40.2 |
| 2 | 46.9 | 48.8 | -2.0** | 45.0 | -2.7** | -5.6 |
| 3 | 57.9 | 56.9 | 1.0 | 57.7 | 1.3 | 2.3 |
| 4 | 62.9 | 60.5 | 2.4** | 63.2 | 3.2** | 5.4 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1** | 11.9 | 16.8 | -4.9*** | 9.7 | -6.5*** | -40.1 |
| 2 | 21.5 | 22.3 | -0.8 | 20.6 | -1.0 | -4.8 |
| 3 | 27.1 | 26.2 | 0.8* | 27.0 | 1.1* | 4.3 |
| 4 | 29.9 | 28.7 | 1.2** | 30.1 | 1.6** | 5.4 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1 | 47.7 | 72.8 | -25.2*** | 33.0 | -33.6*** | -50.5 |
| 2*** | 61.9 | 99.0 | -37.1*** | 43.9 | -49.6*** | -53.1 |
| 3** | 84.0 | 111.8 | -27.9*** | 68.3 | -37.2*** | -35.3 |
| 4** | 102.1 | 121.9 | -19.9*** | 88.6 | -26.5*** | -23.0 |
| 5 | 122.4 | 133.1 | -10.7** | 112.6 | -14.3** | -11.3 |
| 6 | 143.1 | 144.9 | -1.8 | 136.0 | -2.5 | -1.8 |
| 7 | 160.3 | 156.2 | 4.1 | 153.4 | 5.5 | 3.7 |
| 8 | 173.8 | 164.2 | 9.6* | 167.2 | 12.8* | 8.3 |
| 9 | 186.3 | 177.5 | 8.7* | 180.9 | 11.6* | 6.9 |
| 10 | 196.8 | 184.0 | 12.8** | 195.3 | 17.1** | 9.6 |
| 11 | 210.3 | 193.8 | 16.5*** | 208.7 | 22.0*** | 11.8 |
| 12 | 221.6 | 201.3 | 20.3*** | 220.5 | 27.1*** | 14.0 |
| 13 | 231.5 | 209.5 | 22.0*** | 232.3 | 29.4*** | 14.5 |
| 14 | 237.6 | 220.7 | 16.8*** | 238.3 | 22.5*** | 10.4 |
| 15 | 241.8 | 226.8 | 15.0** | 243.3 | 20.1** | 9.0 |
| 16 | 245.3 | 226.3 | 19.0*** | 245.9 | 25.3*** | 11.5 |

TABLE D.8 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 73.5 | 101.5 | -27.9*** | 59.3 | -37.3*** | -38.6 |
| 2 | 149.7 | 150.2 | -0.5 | 141.8 | -0.7 | -0.5 |
| 3 | 203.3 | 187.7 | 15.7*** | 200.8 | 20.9*** | 11.6 |
| 4 | 238.4 | 222.2 | 16.2*** | 238.6 | 21.6*** | 10.0 |
| Average Total Earnings per Week (in 1995 Dollars) | 163.1 | 160.4 | 2.7 | 157.8 | 3.6 | 2.3 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.83 | 7.64 | 0.20* | 7.74 | 0.26* | 3.5 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance*** | 58.8 | 56.2 | 2.6* | 59.7 | 3.5* | 6.2 |
| Paid sick leave | 47.5 | 45.9 | 1.7 | 47.9 | 2.2 | 4.9 |
| Paid vacation*** | 62.8 | 63.1 | -0.3 | 62.5 | -0.4 | -0.6 |
| Retirement or pension benefits*** | 49.8 | 46.9 | 2.9* | 50.7 | 3.9* | 8.3 |
| **Sample Size** | **3,373** | **2,581** | **5,954** | **2,542** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three subgroups of residential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.9

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE RESIDENTIAL DESIGNEES WITHOUT CHILDREN

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 31.9 | 42.8 | -10.9*** | 26.1 | -15.1*** | -36.7 |
| 2** | 30.6 | 47.4 | -16.8*** | 23.2 | -23.3*** | -50.2 |
| 3* | 40.0 | 52.7 | -12.7*** | 35.0 | -17.6*** | -33.5 |
| 4 | 47.7 | 58.9 | -11.2*** | 44.3 | -15.6*** | -26.0 |
| 5 | 48.9 | 56.9 | -8.0*** | 46.9 | -11.1*** | -19.1 |
| 6 | 48.3 | 53.3 | -5.0** | 47.0 | -6.9** | -12.9 |
| 7 | 52.2 | 53.8 | -1.6 | 52.0 | -2.2 | -4.1 |
| 8 | 57.2 | 54.5 | 2.7 | 58.5 | 3.7 | 6.7 |
| 9 | 59.9 | 57.5 | 2.4 | 61.6 | 3.3 | 5.6 |
| 10 | 60.4 | 59.5 | 0.9 | 61.9 | 1.3 | 2.1 |
| 11 | 62.7 | 58.7 | 4.1** | 63.0 | 5.6** | 9.8 |
| 12 | 62.6 | 58.1 | 4.5** | 62.0 | 6.3** | 11.2 |
| 13 | 63.2 | 57.7 | 5.5*** | 63.0 | 7.7*** | 13.9 |
| 14 | 62.8 | 58.0 | 4.8** | 62.9 | 6.7** | 11.8 |
| 15 | 65.9 | 59.2 | 6.7*** | 65.3 | 9.3*** | 16.7 |
| 16 | 68.7 | 64.4 | 4.3** | 68.1 | 5.9** | 9.5 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1** | 25.6 | 37.7 | -12.1*** | 20.5 | -16.9*** | -45.1 |
| 2 | 40.9 | 44.8 | -3.9*** | 39.8 | -5.5*** | -12.1 |
| 3 | 50.4 | 48.2 | 2.2 | 50.7 | 3.1 | 6.5 |
| 4 | 55.7 | 51.1 | 4.6*** | 55.2 | 6.4*** | 13.0 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1** | 10.0 | 14.2 | -4.2*** | 8.0 | -5.9*** | -42.4 |
| 2 | 16.8 | 18.7 | -1.9*** | 16.5 | -2.6*** | -13.7 |
| 3 | 21.0 | 19.8 | 1.2* | 21.4 | 1.7* | 8.6 |
| 4 | 23.6 | 21.7 | 1.9** | 23.4 | 2.7** | 13.0 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1 | 32.5 | 54.1 | -21.6*** | 20.7 | -30.0*** | -59.2 |
| 2*** | 43.9 | 72.8 | -28.9*** | 29.3 | -40.2*** | -57.8 |
| 3** | 61.5 | 80.7 | -19.2*** | 48.4 | -26.6*** | -35.5 |
| 4** | 74.1 | 84.8 | -10.7** | 64.4 | -14.8** | -18.7 |
| 5 | 85.3 | 96.6 | -11.3** | 79.6 | -15.7** | -16.5 |
| 6 | 97.6 | 108.5 | -11.0* | 93.7 | -15.3* | -14.0 |
| 7 | 114.5 | 113.1 | 1.4 | 112.7 | 1.9 | 1.7 |
| 8 | 121.5 | 121.8 | -0.4 | 122.4 | -0.5 | -0.4 |
| 9 | 130.8 | 122.6 | 8.2 | 134.6 | 11.4 | 9.2 |
| 10 | 132.7 | 122.4 | 10.3* | 136.9 | 14.3* | 11.7 |
| 11 | 147.0 | 128.2 | 18.8*** | 148.1 | 26.1*** | 21.4 |
| 12 | 151.6 | 138.1 | 13.5* | 152.9 | 18.7* | 14.0 |
| 13 | 162.2 | 150.6 | 11.7 | 160.8 | 16.2 | 11.2 |
| 14 | 163.7 | 151.2 | 12.4* | 162.7 | 17.2* | 11.8 |
| 15 | 169.8 | 151.0 | 18.7*** | 164.7 | 26.0*** | 18.8 |
| 16 | 173.5 | 157.5 | 16.0** | 168.7 | 22.2** | 15.1 |

TABLE D.9 (continued)

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 52.9 | 73.1 | -20.2*** | 41.1 | -28.1*** | -40.6 |
| 2 | 104.5 | 110.2 | -5.6 | 102.1 | -7.8 | -7.1 |
| 3 | 138.3 | 127.5 | 10.8** | 140.1 | 15.0** | 12.0 |
| 4 | 167.6 | 152.3 | 15.3** | 164.5 | 21.3** | 14.8 |
| Average Total Earnings per Week (in 1995 Dollars) | 111.3 | 112.1 | -0.7 | 108.2 | -1.0 | -0.9 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 6.95 | 6.78 | 0.17 | 6.87 | 0.24 | 3.6 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance*** | 55.6 | 46.5 | 9.1*** | 56.6 | 12.6*** | 28.6 |
| Paid sick leave | 46.3 | 40.9 | 5.4** | 46.6 | 7.5** | 19.2 |
| Paid vacation*** | 62.8 | 53.5 | 9.2*** | 62.7 | 12.8*** | 25.7 |
| Retirement or pension benefits*** | 45.5 | 34.7 | 10.9*** | 47.8 | 15.1*** | 46.0 |
| **Sample Size** | **1,710** | **957** | **2,667** | **1,249** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three subgroups of residential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.10

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE RESIDENTIAL DESIGNEES WITH CHILDREN

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 26.7 | 34.9 | -8.1** | 23.0 | -12.6** | -35.4 |
| 2** | 32.1 | 37.2 | -5.1 | 26.6 | -8.0 | -23.0 |
| 3* | 38.5 | 42.1 | -3.7 | 36.9 | -5.7 | -13.4 |
| 4 | 42.6 | 48.1 | -5.5 | 41.4 | -8.5 | -17.1 |
| 5 | 42.1 | 49.3 | -7.1* | 41.5 | -11.1* | -21.1 |
| 6 | 43.8 | 43.5 | 0.4 | 44.2 | 0.6 | 1.3 |
| 7 | 46.2 | 42.8 | 3.4 | 46.2 | 5.2 | 12.8 |
| 8 | 49.6 | 51.6 | -2.0 | 49.2 | -3.1 | -5.9 |
| 9 | 54.1 | 54.8 | -0.7 | 52.6 | -1.1 | -2.1 |
| 10 | 56.2 | 58.3 | -2.1 | 55.9 | -3.3 | -5.6 |
| 11 | 57.4 | 60.3 | -2.9 | 58.4 | -4.4 | -7.1 |
| 12 | 57.8 | 56.3 | 1.5 | 57.6 | 2.3 | 4.1 |
| 13 | 61.0 | 55.9 | 5.0 | 66.9 | 7.8 | 13.2 |
| 14 | 61.9 | 60.2 | 1.7 | 64.8 | 2.6 | 4.2 |
| 15 | 60.8 | 61.7 | -0.9 | 65.8 | -1.4 | -2.0 |
| 16 | 65.5 | 65.2 | 0.2 | 66.8 | 0.4 | 0.6 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1** | 24.6 | 29.1 | -4.5* | 21.4 | -6.9* | -24.5 |
| 2 | 36.7 | 37.6 | -1.0 | 36.2 | -1.5 | -4.0 |
| 3 | 46.9 | 47.4 | -0.5 | 47.6 | -0.8 | -1.6 |
| 4 | 51.7 | 49.4 | 2.3 | 55.2 | 3.6 | 6.9 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1** | 9.8 | 11.4 | -1.7 | 8.2 | -2.6 | -23.9 |
| 2 | 15.9 | 15.2 | 0.7 | 15.4 | 1.1 | 7.6 |
| 3 | 19.9 | 19.1 | 0.8 | 20.7 | 1.2 | 6.3 |
| 4 | 22.3 | 19.8 | 2.5 | 23.6 | 3.8 | 19.3 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1 | 32.4 | 51.0 | -18.5** | 28.1 | -28.7** | -50.5 |
| 2*** | 53.3 | 59.2 | -5.9 | 39.6 | -9.1 | -18.8 |
| 3** | 69.1 | 65.2 | 3.9 | 57.8 | 6.0 | 11.6 |
| 4** | 78.2 | 67.5 | 10.7 | 74.8 | 16.6 | 28.5 |
| 5 | 88.8 | 80.6 | 8.2 | 86.5 | 12.7 | 17.3 |
| 6 | 96.7 | 83.0 | 13.7 | 95.0 | 21.3 | 28.9 |
| 7 | 105.9 | 86.5 | 19.4 | 107.0 | 30.1 | 39.1 |
| 8 | 112.3 | 105.7 | 6.6 | 113.5 | 10.2 | 9.9 |
| 9 | 125.5 | 113.5 | 12.1 | 126.4 | 18.7 | 17.4 |
| 10 | 126.1 | 122.2 | 3.8 | 129.0 | 5.9 | 4.8 |
| 11 | 138.5 | 130.5 | 8.0 | 147.9 | 12.4 | 9.2 |
| 12 | 151.5 | 129.9 | 21.7 | 161.6 | 33.6 | 26.2 |
| 13 | 154.7 | 132.7 | 22.0 | 176.4 | 34.1 | 24.0 |
| 14 | 159.7 | 133.7 | 26.0* | 172.9 | 40.2* | 30.3 |
| 15 | 158.4 | 142.6 | 15.8 | 170.9 | 24.5 | 16.7 |
| 16 | 164.0 | 148.4 | 15.5 | 176.8 | 24.1 | 15.8 |

TABLE D.10 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1*** | 58.6 | 60.6 | -2.1 | 50.6 | -3.2 | -5.9 |
| 2 | 101.6 | 88.0 | 13.5 | 101.1 | 21.0 | 26.2 |
| 3 | 133.8 | 119.9 | 13.9 | 140.3 | 21.6 | 18.2 |
| 4 | 159.9 | 140.2 | 19.7 | 174.3 | 30.5 | 21.2 |
| Average Total Earnings per Week (in 1995 Dollars) | 111.1 | 99.7 | 11.4 | 112.7 | 17.7 | 18.7 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.10 | 6.88 | 0.22 | 7.30 | 0.34 | 4.8 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance*** | 49.3 | 59.8 | -10.5* | 50.6 | -16.3* | -23.8 |
| Paid sick leave | 44.9 | 40.5 | 4.4 | 49.0 | 6.8 | 16.0 |
| Paid vacation*** | 58.2 | 63.5 | -5.3 | 58.8 | -8.2 | -12.3 |
| Retirement or pension benefits*** | 38.2 | 47.9 | -9.7* | 40.6 | -15.0* | -26.4 |
| **Sample Size** | **387** | **206** | **593** | **257** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three subgroups of residential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.11

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR MALE NONRESIDENTIAL DESIGNEES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 41.6 | 47.6 | -6.0 | 39.1 | -8.7 | -18.1 |
| 2 | 44.8 | 52.2 | -7.4* | 40.8 | -10.7* | -20.8 |
| 3 | 52.2 | 54.6 | -2.4 | 47.2 | -3.5 | -6.8 |
| 4 | 60.6 | 62.1 | -1.5 | 58.4 | -2.1 | -3.5 |
| 5 | 59.8 | 58.2 | 1.6 | 59.9 | 2.4 | 4.1 |
| 6* | 60.0 | 57.1 | 2.9 | 62.1 | 4.2 | 7.2 |
| 7 | 63.6 | 60.2 | 3.4 | 67.1 | 4.9 | 7.9 |
| 8 | 67.5 | 61.1 | 6.4 | 70.2 | 9.3 | 15.2 |
| 9 | 69.3 | 64.0 | 5.3 | 75.0 | 7.7 | 11.4 |
| 10 | 68.8 | 64.5 | 4.3 | 72.4 | 6.1 | 9.3 |
| 11 | 73.0 | 67.4 | 5.6 | 78.3 | 8.1 | 11.5 |
| 12 | 71.5 | 67.8 | 3.7 | 75.4 | 5.3 | 7.5 |
| 13* | 70.9 | 67.6 | 3.4 | 73.0 | 4.8 | 7.1 |
| 14 | 71.6 | 70.0 | 1.5 | 74.7 | 2.2 | 3.0 |
| 15 | 73.6 | 67.3 | 6.3* | 75.9 | 9.1* | 13.7 |
| 16 | 74.3 | 71.8 | 2.6 | 75.1 | 3.7 | 5.1 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1 | 37.1 | 44.0 | -6.9** | 34.1 | -9.9** | -22.5 |
| 2* | 53.3 | 51.2 | 2.1 | 54.7 | 3.1 | 6.0 |
| 3 | 61.2 | 58.7 | 2.6 | 65.2 | 3.7 | 6.0 |
| 4 | 65.0 | 60.1 | 4.9 | 67.7 | 7.1 | 11.6 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1 | 15.2 | 19.0 | -3.8*** | 13.2 | -5.5*** | -29.4 |
| 2 | 23.3 | 23.1 | 0.2 | 23.6 | 0.3 | 1.4 |
| 3 | 27.6 | 27.3 | 0.4 | 29.8 | 0.6 | 1.9 |
| 4* | 29.7 | 28.1 | 1.5 | 30.8 | 2.2 | 7.8 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1 | 65.9 | 87.7 | -21.8** | 51.0 | -31.4** | -38.1 |
| 2 | 87.5 | 106.1 | -18.6 | 74.7 | -26.8 | -26.4 |
| 3 | 110.3 | 124.8 | -14.5 | 98.0 | -20.8 | -17.5 |
| 4 | 122.2 | 131.8 | -9.6 | 116.8 | -13.8 | -10.6 |
| 5 | 139.3 | 144.8 | -5.5 | 135.8 | -7.9 | -5.5 |
| 6** | 166.3 | 152.4 | 13.9 | 165.5 | 20.1 | 13.8 |
| 7 | 178.5 | 164.9 | 13.6 | 184.0 | 19.6 | 11.9 |
| 8 | 196.2 | 166.9 | 29.3 | 206.9 | 42.2 | 25.6 |
| 9 | 203.3 | 181.7 | 21.6 | 217.6 | 31.1 | 16.7 |
| 10 | 204.0 | 189.6 | 14.4 | 218.2 | 20.7 | 10.5 |
| 11 | 231.0 | 220.0 | 11.0 | 246.4 | 15.8 | 6.9 |
| 12* | 250.5 | 225.0 | 25.5 | 264.6 | 36.7 | 16.1 |
| 13** | 254.4 | 228.1 | 26.3 | 264.4 | 37.9 | 16.7 |
| 14 | 261.0 | 220.5 | 40.5* | 266.2 | 58.3* | 28.0 |
| 15 | 259.5 | 207.3 | 52.2** | 269.1 | 75.1** | 38.7 |
| 16 | 261.4 | 215.2 | 46.2** | 261.3 | 66.5** | 34.2 |

TABLE D.11 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1 | 96.0 | 113.5 | -17.5* | 85.1 | -25.2* | -22.9 |
| 2 | 164.0 | 155.5 | 8.5 | 164.7 | 12.3 | 8.1 |
| 3 | 218.9 | 205.6 | 13.3 | 235.4 | 19.2 | 8.9 |
| 4* | 258.6 | 220.1 | 38.5** | 265.3 | 55.4** | 26.4 |
| Average Total Earnings per Week (in 1995 Dollars) | 173.1 | 165.6 | 7.4 | 174.4 | 10.7 | 6.5 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 8.36 | 7.37 | 0.99** | 8.30 | 1.43** | 20.7 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 59.7 | 55.5 | 4.1 | 58.0 | 6.0 | 11.5 |
| Paid sick leave | 49.2 | 44.4 | 4.8 | 45.9 | 7.0 | 17.9 |
| Paid vacation | 64.2 | 58.6 | 5.6 | 63.4 | 8.1 | 14.6 |
| Retirement or pension benefits* | 53.5 | 38.4 | 15.0*** | 54.7 | 21.6*** | 65.2 |
| **Sample Size** | **368** | **206** | **574** | **257** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three subgroups of nonresidential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.12

## IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE NONRESIDENTIAL DESIGNEES WITHOUT CHILDREN

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 44.0 | 47.9 | -3.9 | 39.3 | -6.1 | -13.4 |
| 2 | 48.9 | 53.2 | -4.3 | 43.0 | -6.8 | -13.6 |
| 3 | 50.9 | 57.5 | -6.6 | 47.0 | -10.3 | -17.9 |
| 4 | 56.7 | 57.3 | -0.6 | 55.1 | -0.9 | -1.6 |
| 5 | 52.6 | 56.7 | -4.1 | 53.6 | -6.4 | -10.6 |
| 6* | 53.4 | 60.1 | -6.7 | 55.2 | -10.5 | -16.0 |
| 7 | 57.3 | 60.4 | -3.1 | 56.1 | -4.8 | -7.9 |
| 8 | 61.7 | 58.1 | 3.6 | 62.0 | 5.6 | 10.0 |
| 9 | 61.9 | 56.9 | 5.0 | 64.4 | 7.8 | 13.8 |
| 10 | 66.3 | 57.6 | 8.7** | 68.6 | 13.6** | 24.7 |
| 11 | 66.5 | 62.9 | 3.5 | 65.5 | 5.5 | 9.2 |
| 12 | 66.0 | 66.3 | -0.3 | 65.0 | -0.5 | -0.8 |
| 13* | 62.4 | 70.4 | -8.0* | 63.3 | -12.6* | -16.6 |
| 14 | 65.9 | 70.5 | -4.6 | 65.3 | -7.1 | -9.8 |
| 15 | 65.8 | 67.8 | -2.0 | 67.3 | -3.2 | -4.5 |
| 16 | 70.2 | 69.5 | 0.7 | 71.0 | 1.1 | 1.6 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1 | 38.3 | 41.5 | -3.3 | 33.3 | -5.1 | -13.3 |
| 2* | 45.8 | 51.0 | -5.2 | 45.8 | -8.1 | -15.1 |
| 3 | 55.7 | 52.5 | 3.3 | 55.9 | 5.2 | 10.2 |
| 4 | 58.8 | 61.7 | -2.9 | 59.9 | -4.5 | -7.0 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1 | 14.7 | 16.2 | -1.6 | 12.5 | -2.4 | -16.4 |
| 2 | 18.5 | 20.5 | -2.0 | 19.2 | -3.1 | -13.8 |
| 3 | 22.9 | 22.0 | 0.8 | 23.7 | 1.3 | 5.8 |
| 4* | 24.5 | 26.4 | -1.9 | 25.2 | -2.9 | -10.4 |
| Average Earnings Per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1 | 59.7 | 67.1 | -7.4 | 52.8 | -11.6 | -18.0 |
| 2 | 80.5 | 82.4 | -1.9 | 68.2 | -3.0 | -4.2 |
| 3 | 88.8 | 97.7 | -8.9 | 75.9 | -13.9 | -15.5 |
| 4 | 91.9 | 97.0 | -5.1 | 82.8 | -8.1 | -8.9 |
| 5 | 98.9 | 116.5 | -17.6 | 103.6 | -27.5 | -21.0 |
| 6** | 109.6 | 131.6 | -22.0* | 113.0 | -34.4* | -23.3 |
| 7 | 122.0 | 135.8 | -13.8 | 126.6 | -21.6 | -14.6 |
| 8 | 129.6 | 135.8 | -6.2 | 132.3 | -9.7 | -6.8 |
| 9 | 140.8 | 136.2 | 4.6 | 145.1 | 7.2 | 5.2 |
| 10 | 143.7 | 143.1 | 0.6 | 149.0 | 0.9 | 0.6 |
| 11 | 162.6 | 159.2 | 3.4 | 162.3 | 5.3 | 3.4 |
| 12* | 165.8 | 180.5 | -14.7 | 169.0 | -23.0 | -12.0 |
| 13** | 173.1 | 197.5 | -24.4 | 178.4 | -38.1 | -17.6 |
| 14 | 181.2 | 195.7 | -14.5 | 185.0 | -22.7 | -10.9 |
| 15 | 189.2 | 194.3 | -5.1 | 193.6 | -8.0 | -4.0 |
| 16 | 192.7 | 195.1 | -2.4 | 195.3 | -3.8 | -1.9 |

TABLE D.12 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1 | 79.9 | 86.3 | -6.4 | 68.8 | -10.0 | -12.7 |
| 2 | 114.7 | 129.7 | -15.0 | 117.8 | -23.5 | -16.6 |
| 3 | 151.8 | 154.1 | -2.3 | 154.7 | -3.6 | -2.3 |
| 4* | 182.6 | 194.6 | -12.0 | 186.7 | -18.8 | -9.1 |
| Average Total Earnings per Week (in 1995 Dollars) | 126.7 | 136.8 | -10.1 | 125.9 | -15.8 | -11.1 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.28 | 7.12 | 0.16 | 7.24 | 0.25 | 3.6 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 55.3 | 53.2 | 2.1 | 60.3 | 3.3 | 5.7 |
| Paid sick leave | 46.6 | 45.2 | 1.4 | 51.6 | 2.2 | 4.5 |
| Paid vacation | 62.4 | 62.5 | -0.1 | 62.3 | -0.2 | -0.3 |
| Retirement or pension benefits* | 46.4 | 47.8 | -1.4 | 48.2 | -2.2 | -4.3 |
| **Sample Size** | **350** | **189** | **539** | **228** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three subgroups of nonresidential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE D.13

IMPACTS ON EMPLOYMENT AND EARNINGS FOR FEMALE NONRESIDENTIAL DESIGNEES WITH CHILDREN

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Employed, by Quarter | | | | | | |
| 1 | 25.9 | 31.3 | -5.3* | 19.1 | -8.9* | -31.9 |
| 2 | 32.1 | 35.5 | -3.4 | 24.5 | -5.6 | -18.7 |
| 3 | 41.8 | 41.0 | 0.8 | 33.6 | 1.3 | 4.0 |
| 4 | 45.5 | 42.7 | 2.8 | 40.2 | 4.7 | 13.3 |
| 5 | 49.5 | 43.6 | 5.9* | 45.1 | 9.9* | 28.1 |
| 6* | 50.2 | 44.8 | 5.4 | 47.2 | 9.0 | 23.7 |
| 7 | 51.8 | 48.4 | 3.5 | 50.3 | 5.8 | 13.1 |
| 8 | 55.8 | 51.5 | 4.3 | 55.2 | 7.2 | 15.0 |
| 9 | 60.4 | 53.8 | 6.7** | 61.3 | 11.2** | 22.3 |
| 10 | 64.6 | 56.5 | 8.1** | 65.6 | 13.6** | 26.1 |
| 11 | 66.7 | 56.6 | 10.1*** | 66.9 | 16.9*** | 33.8 |
| 12 | 66.7 | 58.1 | 8.7*** | 64.4 | 14.5*** | 29.1 |
| 13* | 67.4 | 63.3 | 4.2 | 66.3 | 7.0 | 11.8 |
| 14 | 68.0 | 63.8 | 4.2 | 66.6 | 7.0 | 11.8 |
| 15 | 68.5 | 63.4 | 5.1 | 69.5 | 8.5 | 14.0 |
| 16 | 71.2 | 67.3 | 3.9 | 69.3 | 6.5 | 10.3 |
| Average Percentage of Weeks Employed, by Year | | | | | | |
| 1 | 26.6 | 28.6 | -2.0 | 20.4 | -3.4 | -14.2 |
| 2* | 42.8 | 38.5 | 4.3* | 40.4 | 7.2* | 21.5 |
| 3 | 55.2 | 46.8 | 8.4*** | 55.8 | 14.1*** | 33.7 |
| 4 | 60.5 | 54.3 | 6.2** | 59.5 | 10.4** | 21.3 |
| Average Hours per Week Employed, by Year | | | | | | |
| 1 | 9.9 | 10.6 | -0.8 | 7.3 | -1.3 | -14.7 |
| 2 | 17.1 | 15.4 | 1.7 | 16.4 | 2.9 | 21.2 |
| 3 | 22.4 | 18.6 | 3.8*** | 22.7 | 6.4*** | 39.4 |
| 4* | 25.3 | 22.1 | 3.2*** | 24.9 | 5.3*** | 27.3 |
| Average Earnings per Week, by Quarter (in 1995 Dollars) | | | | | | |
| 1 | 40.2 | 44.7 | -4.4 | 30.0 | -7.4 | -19.8 |
| 2 | 52.8 | 57.7 | -4.9 | 34.7 | -8.2 | -19.1 |
| 3 | 65.4 | 77.2 | -11.9 | 47.5 | -19.8 | -29.5 |
| 4 | 76.1 | 75.6 | 0.4 | 65.8 | 0.7 | 1.1 |
| 5 | 92.4 | 81.9 | 10.6 | 85.0 | 17.7 | 26.2 |
| 6** | 117.6 | 96.8 | 20.8* | 115.5 | 34.8* | 43.1 |
| 7 | 123.2 | 107.4 | 15.8 | 126.4 | 26.4 | 26.4 |
| 8 | 129.6 | 123.4 | 6.3 | 131.0 | 10.5 | 8.7 |
| 9 | 144.0 | 122.9 | 21.1* | 150.3 | 35.3* | 30.7 |
| 10 | 147.4 | 122.7 | 24.7** | 156.7 | 41.3** | 35.8 |
| 11 | 162.4 | 137.1 | 25.3** | 170.4 | 42.4** | 33.1 |
| 12* | 181.3 | 147.9 | 33.4*** | 183.8 | 55.9*** | 43.7 |
| 13** | 184.5 | 160.8 | 23.7* | 186.5 | 39.6* | 27.0 |
| 14 | 186.1 | 168.6 | 17.5 | 185.6 | 29.2 | 18.7 |
| 15 | 185.1 | 166.3 | 18.8 | 186.9 | 31.4 | 20.2 |
| 16 | 190.3 | 166.6 | 23.7* | 186.7 | 39.7* | 27.0 |

TABLE D.13 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Earnings per Week, by Year (in 1995 Dollars) | | | | | | |
| 1 | 58.7 | 64.1 | -5.4 | 44.8 | -9.0 | -16.8 |
| 2 | 111.2 | 102.9 | 8.3 | 106.9 | 13.9 | 14.9 |
| 3 | 157.8 | 133.1 | 24.6** | 164.4 | 41.2** | 33.5 |
| 4* | 186.5 | 165.0 | 21.4* | 185.7 | 35.9* | 23.9 |
| Average Total Earnings per Week (in 1995 Dollars) | 123.5 | 114.4 | 9.1 | 120.2 | 15.3 | 14.5 |
| Average Hourly Wage in the Most Recent Job in Quarter 16 (in 1995 Dollars) | 7.35 | 7.13 | 0.23 | 7.39 | 0.38 | 5.4 |
| Job Benefits Available in the Most Recent Job in Quarter 16 (Percentage) | | | | | | |
| Health insurance | 58.3 | 63.5 | -5.2 | 55.7 | -8.8 | -13.6 |
| Paid sick leave | 50.0 | 50.0 | 0.0 | 49.8 | 0.0 | 0.0 |
| Paid vacation | 67.0 | 64.8 | 2.2 | 66.3 | 3.7 | 5.8 |
| Retirement or pension benefits* | 50.5 | 45.3 | 5.2 | 51.5 | 8.7 | 20.3 |
| **Sample Size** | **618** | **332** | **950** | **380** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three subgroups of nonresidential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.14

## KEY EMPLOYMENT AND EARNINGS OUTCOMES, BY HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE

| | Percentage Employed in Quarter 16 | | Percentage of Weeks Employed in Year 4 | | Hours per Week Employed in Year 4 | | Earnings per Week in Year 4 (1995 Dollars) | | Hourly Wage on Most Recent Job in Quarter 16 (1995 Dollars) | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Eligible Applicant[b] |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 76.4 | 5.1** | 66.8 | 5.2** | 29.2 | 3.1*** | 231.9 | 32.5*** | 7.80 | 0.18 |
| Had no high school credential | 66.3 | 2.7* | 54.2 | 3.9*** | 25.0 | 1.6*** | 184.1 | 19.4*** | 7.17 | 0.22** |
| (P-value)[c] | | .459 | | .717 | | .338 | | .403 | | .838 |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 69.8 | 3.1** | 58.3 | 3.4*** | 26.3 | 1.6*** | 196.6 | 21.0*** | 7.27 | 0.23*** |
| Ever arrested for nonserious crimes only | 67.2 | 3.2 | 55.1 | 5.4** | 25.3 | 2.8** | 194.1 | 22.7* | 7.51 | 0.15 |
| Ever arrested for serious crimes[d] | 67.3 | -2.1 | 55.8 | -1.1 | 26.4 | -0.6 | 195.3 | 8.2 | 7.34 | 0.16 |
| (P-value)[c] | | .693 | | .520 | | .507 | | .867 | | .924 |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 76.0 | 5.0** | 63.9 | 6.6*** | 30.1 | 3.4*** | 231.3 | 46.2*** | 7.52 | 0.43*** |
| Black non-Hispanic | 63.2 | 4.7*** | 51.8 | 5.4*** | 23.2 | 2.3*** | 166.5 | 22.8*** | 6.98 | 0.21* |
| Hispanic | 72.3 | -3.1 | 61.1 | -1.1 | 27.2 | -0.5 | 217.9 | -15.1 | 7.77 | -0.19 |
| Other[e] | 68.7 | 2.4 | 58.3 | -0.3 | 25.8 | -0.4 | 197.9 | 16.1 | 7.60 | 0.46 |
| (P-value)[c] | | .112 | | .050* | | .077* | | .005*** | | .062* |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 68.2 | 0.7 | 55.5 | 4.8** | 25.2 | 2.2* | 189.6 | 15.7 | 7.33 | 0.06 |
| On or after 3/1/95 (after ZT) | 68.8 | 4.2*** | 57.7 | 4.0*** | 26.2 | 1.9*** | 197.2 | 24.4*** | 7.33 | 0.26*** |
| (P-value)[c] | | .241 | | .784 | | .836 | | .445 | | .271 |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[d] Serious crimes include aggravated assault, murder, robbery, and burglary.

[e] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE D.15

## ESTIMATED IMPACTS PER PARTICIPANT ON EARNINGS PER WEEK IN YEAR 4 ACROSS KEY SUBGROUPS, BY RACE AND ETHNICITY

| Subgroup | White, Non-Hispanic | Black, Non-Hispanic | Hispanic |
|---|---|---|---|
| Age at Application | | | |
| 16 to 17 | 55.3*** | 9.8 | -13.1 |
| 18 to 19 | -9.6 | 13.0 | 0.2 |
| 20 to 24 | 103.2*** | 60.3*** | -34.0 |
| Gender | | | |
| Male | 39.9*** | 25.3*** | -10.1 |
| Female | 55.7*** | 20.2** | -13.2 |
| Education Level at Random Assignment | | | |
| Had a high school diploma or GED | 48.1** | 35.1* | -10.2 |
| Had neither | 46.1*** | 20.1*** | -16.9 |
| Native Language | | | |
| English | n.a. | n.a. | -23.2 |
| Other | n.a. | n.a. | -6.7 |
| Needs a Bilingual Program in Job Corps | | | |
| Yes | n.a. | n.a. | -30.0 |
| No | n.a. | n.a. | -13.6 |
| In a Region with a Large Concentration of Hispanic Students (Regions 2, 6, and 9) | | | |
| Yes | 14.7 | 13.9 | -9.5 |
| No | 53.8*** | 24.7*** | -22.4 |
| Designated for One of 25 Centers with a Large Concentration of Hispanic Students | | | |
| Yes | -5.8 | 13.3 | -25.1 |
| No | 55.2*** | 24.1*** | -2.5 |
| **Sample Size** | **2,982** | **5,541** | **1,961** |

TABLE D.15 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data, and ETA-652 and Supplemental ETA-652 data, for those who completed 48-month interviews.

NOTE: Earnings are in 1995 dollars. All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per Job Corps participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates account for the estimation error in the Job Corps participation and control group crossover rates.

n.a. = Not applicable because the sample size of those whose primary language was not English or who needed a bilingual program in Job Corps were very small.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# APPENDIX E

## SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON PUBLIC ASSISTANCE OUTCOMES

## TABLE E.1

## IMPACTS ON OTHER SOURCES OF INCOME

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Percentage Received Unemployment Insurance (UI) Benefits During the 48 Months After Random Assignment** | 5.6 | 7.1 | -1.5*** | 5.2 | -2.1*** | -28.3 |
| Average Number of Weeks Ever Received UI Benefits | 0.8 | 1.0 | -0.2*** | 0.7 | -0.3*** | -32.1 |
| Average Amount of UI Benefits Ever Received (in Dollars) | 100.6 | 136.9 | -36.3*** | 88.1 | -50.4*** | -36.4 |
| Percentage Received Child Support | | | | | | |
| Before the 12-month interview | 1.8 | 1.8 | 0.0 | 1.5 | 0.0 | 0.2 |
| Before the 30-month interview | 4.1 | 4.0 | 0.1 | 3.4 | 0.1 | 3.0 |
| Before the 48-month interview | 6.6 | 6.2 | 0.4 | 5.9 | 0.5 | 9.3 |
| Average Amount of Child Support Ever Received (in Dollars) | 117.0 | 110.7 | 6.3 | 108 | 8.8 | 8.9 |
| Percentage Ever Received Income from Friends | | | | | | |
| Before the 12-month interview | 11.5 | 11.1 | 0.4 | 11.8 | 0.6 | 5.4 |
| Before the 30-month interview | 17.9 | 18.2 | -0.3 | 18.0 | -0.5 | -2.6 |
| Before the 48-month interview | 23.1 | 23.6 | -0.5 | 23.1 | -0.6 | -2.7 |
| Average Amount of Income Ever Received from Friends (in Dollars) | 258.9 | 252.2 | 6.7 | 250.7 | 9.3 | 3.9 |
| Percentage Received Other Income | | | | | | |
| Before the 12-month interview | 6.5 | 6.7 | -0.2 | 6.6 | -0.3 | -4.0 |
| Before the 30-month interview | 10.9 | 11.0 | -0.1 | 10.9 | -0.2 | -1.8 |
| Before the 48-month interview | 13.8 | 13.9 | -0.1 | 13.9 | -0.1 | -0.5 |
| Average Amount of Other Income Ever Received (in Dollars) | 287.9 | 292.8 | -4.9 | 281.5 | -6.8 | -2.4 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

TABLE E.1 *(continued)*

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[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.2

## IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE FOR MALES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Received Any Benefit (AFDC/TANF, Food Stamps, SSI/SSA, or GA), by Quarter After Random Assignment | | | | | | |
| 1 | 27.8 | 30.0 | -2.1* | 27.0 | -2.9* | -9.6 |
| 2 | 13.5 | 16.2 | -2.7*** | 12.2 | -3.6*** | -22.9 |
| 3 | 13.4 | 16.3 | -2.9*** | 12.3 | -3.9*** | -24.3 |
| 4 | 14.4 | 16.9 | -2.5*** | 13.3 | -3.4*** | -20.4 |
| 5* | 17.3 | 20.2 | -2.9*** | 16.5 | -3.9*** | -19.2 |
| 6 | 11.3 | 13.5 | -2.2*** | 10.7 | -3.0*** | -22.0 |
| 7 | 9.5 | 12.3 | -2.7*** | 8.8 | -3.6*** | -29.2 |
| 8 | 8.7 | 11.5 | -2.8*** | 8.0 | -3.7*** | -31.7 |
| 9 | 8.7 | 11.8 | -3.1*** | 7.9 | -4.2*** | -34.7 |
| 10* | 9.2 | 12.1 | -2.9*** | 8.6 | -4.0*** | -31.4 |
| 11 | 10.2 | 12.9 | -2.7*** | 9.6 | -3.6*** | -27.3 |
| 12 | 6.8 | 8.7 | -1.9*** | 6.2 | -2.5*** | -28.6 |
| 13 | 5.6 | 7.5 | -1.9*** | 5.4 | -2.5*** | -32.0 |
| 14 | 5.2 | 6.6 | -1.4** | 5.1 | -1.9** | -27.0 |
| 15 | 5.2 | 6.9 | -1.7*** | 5.1 | -2.3*** | -30.9 |
| 16 | 5.4 | 7.6 | -2.2*** | 5.1 | -3.0*** | -36.8 |
| Percentage Received Any Benefits, by Year | | | | | | |
| All years | 41.1 | 45.7 | -4.6*** | 40.3 | -6.2*** | -13.4 |
| 1 | 30.6 | 33.6 | -3.0** | 29.7 | -4.0** | -11.8 |
| 2 | 20.8 | 24.4 | -3.6*** | 19.7 | -4.8*** | -19.7 |
| 3 | 12.9 | 16.5 | -3.6*** | 12.3 | -4.9*** | -28.3 |
| 4 | 8.2 | 10.4 | -2.2*** | 8.1 | -3.0*** | -26.9 |
| Average Number of Months Received Any Benefits, by Year | | | | | | |
| All years | 4.5 | 5.8 | -1.2*** | 4.2 | -1.7*** | -28.6 |
| 1 | 1.8 | 2.1 | -0.3*** | 1.6 | -0.4*** | -21.6 |
| 2 | 1.2 | 1.5 | -0.3*** | 1.2 | -0.4*** | -25.4 |
| 3 | 0.9 | 1.2 | -0.3*** | 0.9 | -0.4*** | -32.4 |
| 4 | 0.6 | 0.8 | -0.2*** | 0.6 | -0.3*** | -35.5 |
| Average Amount of Any Benefits Received, by Year (in Dollars) | | | | | | |
| All years | 1,613.7 | 2,075.6 | -461.9*** | 1,461.8 | -619.9*** | -29.8 |
| 1 | 614.3 | 730.1 | -115.9*** | 552.1 | -155.5*** | -22.0 |
| 2 | 467.3 | 577.0 | -109.6*** | 427.3 | -147.2*** | -25.6 |
| 3 | 366.6 | 481.3 | -114.7*** | 329.2 | -154.0*** | -31.9 |
| 4 | 211.8 | 302.8 | -91.0*** | 192.7 | -122.2*** | -38.8 |
| Percentage Received AFDC/TANF Benefits, by Year | | | | | | |
| All years | 18.7 | 20.7 | -2.0* | 18.5 | -2.6* | -12.4 |
| 1 | 15.1 | 15.9 | -0.8 | 14.7 | -1.0 | -6.6 |
| 2 | 7.7 | 9.1 | -1.3* | 7.3 | -1.8* | -19.5 |
| 3 | 3.8 | 5.3 | -1.5*** | 3.7 | -2.0*** | -35.5 |
| 4 | 1.7 | 2.6 | -1.0*** | 1.8 | -1.3*** | -42.4 |
| Average Number of Months Ever Received AFDC/TANF Benefits | 1.5 | 1.9 | -0.4*** | 1.5 | -0.5*** | -26.2 |

TABLE E.2 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Amount of AFDC/TANF Benefits Ever Received (in Dollars) | 427.3 | 537.0 | -109.7** | 389.9 | -147.3** | -27.4 |
| Percentage Received Food Stamp Benefits, by Year | | | | | | |
| All years | 31.4 | 35.5 | -4.1*** | 30.7 | -5.5*** | -15.2 |
| 1 | 23.7 | 25.4 | -1.7 | 22.9 | -2.2 | -8.9 |
| 2 | 12.9 | 14.8 | -1.8** | 11.9 | -2.5** | -17.2 |
| 3 | 7.9 | 10.5 | -2.7*** | 7.6 | -3.6*** | -32.0 |
| 4 | 4.8 | 6.4 | -1.6*** | 4.8 | -2.2*** | -31.4 |
| Average Number of Months Ever Received Food Stamp Benefits | 2.5 | 3.1 | -0.6*** | 2.3 | -0.8*** | -25.3 |
| Average Amount of Food Stamp Benefits Ever Received (in Dollars) | 467.3 | 561.2 | -93.9*** | 421.4 | -126.0*** | -23.0 |
| Covered by Public Health Insurance | | | | | | |
| At the 30-month interview | 23.2 | 24.9 | -1.7 | 22.9 | -2.3 | -9.3 |
| At the 48-month interview | 22.3 | 24.6 | -2.3** | 22.3 | -3.1** | -12.2 |
| Percentage Ever Received General Assistance Benefits | 2.8 | 3.9 | -1.1** | 2.6 | -1.5** | -36.2 |
| Average Amount of General Assistance Benefits Ever Received (in Dollars) | 56.0 | 82.4 | -26.5* | 42.1 | -35.5* | -45.8 |
| Percentage Ever Received SSI/SSA Benefits | 8.6 | 9.9 | -1.3* | 8.1 | -1.8* | -17.8 |
| Average Amount of SSI/SSA Benefits Ever Received (in Dollars) | 688.5 | 891.2 | -202.7** | 602.5 | -272.0** | -31.1 |
| Percentage Lived in Public Housing | | | | | | |
| At the 30-month interview | 11.9 | 12.6 | -0.7 | 12.3 | -1.0 | -7.4 |
| At the 48-month interview | 9.2 | 9.7 | -0.5 | 9.2 | -0.6 | -6.4 |
| Percentage Ever Received Child Support | 0.3 | 0.6 | -0.3* | 0.3 | -0.4* | -57.6 |
| **Sample Size** | **3,741** | **2,787** | **6,528** | **2,799** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

TABLE E.2 *(continued)*

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[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three gender subgroups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the differences between the weighted means for program and control group members divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.3

## IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE FOR FEMALES WITHOUT CHILDREN

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Received Any Benefit (AFDC/TANF, Food Stamps, SSI/SSA, or GA), by Quarter After Random Assignment | | | | | | |
| 1 | 36.4 | 38.0 | -1.6 | 35.9 | -2.2 | -5.8 |
| 2 | 19.2 | 22.4 | -3.2** | 17.0 | -4.5** | -20.8 |
| 3 | 20.0 | 23.7 | -3.7** | 17.7 | -5.2** | -22.6 |
| 4 | 21.9 | 25.9 | -3.9** | 19.8 | -5.5** | -21.8 |
| 5* | 29.2 | 31.9 | -2.7 | 26.8 | -3.8 | -12.3 |
| 6 | 23.1 | 24.9 | -1.8 | 21.1 | -2.5 | -10.8 |
| 7 | 20.9 | 24.4 | -3.4** | 19.0 | -4.8** | -20.2 |
| 8 | 22.1 | 24.4 | -2.3 | 20.8 | -3.2 | -13.4 |
| 9 | 24.0 | 25.5 | -1.5 | 22.4 | -2.2 | -8.8 |
| 10* | 26.6 | 26.6 | 0.0 | 24.6 | -0.1 | -0.2 |
| 11 | 28.8 | 30.2 | -1.4 | 28.0 | -2.0 | -6.5 |
| 12 | 24.0 | 24.6 | -0.6 | 23.1 | -0.9 | -3.6 |
| 13 | 24.0 | 23.3 | 0.6 | 23.6 | 0.9 | 3.9 |
| 14 | 24.2 | 23.5 | 0.7 | 23.9 | 1.0 | 4.4 |
| 15 | 25.4 | 24.7 | 0.7 | 25.8 | 0.9 | 3.7 |
| 16 | 27.4 | 27.0 | 0.3 | 28.1 | 0.5 | 1.7 |
| Percentage Received Any Benefits, by Year | | | | | | |
| All years | 65.1 | 66.1 | -1.1 | 64.7 | -1.5 | -2.2 |
| 1 | 41.7 | 44.2 | -2.5 | 40.4 | -3.5 | -7.9 |
| 2 | 36.9 | 40.1 | -3.2* | 34.5 | -4.5* | -11.5 |
| 3 | 35.1 | 36.7 | -1.6 | 34.0 | -2.2 | -6.2 |
| 4 | 33.4 | 32.3 | 1.1 | 33.8 | 1.6 | 4.9 |
| Average Number of Months Received Any Benefits, by Year | | | | | | |
| All years | 10.9 | 11.8 | -0.9* | 10.4 | -1.2* | -10.7 |
| 1 | 2.5 | 2.9 | -0.4*** | 2.3 | -0.6*** | -21.3 |
| 2 | 2.5 | 2.9 | -0.3** | 2.3 | -0.5** | -16.3 |
| 3 | 2.8 | 2.9 | -0.1 | 2.7 | -0.2 | -5.6 |
| 4 | 2.8 | 2.8 | 0.0 | 2.8 | 0.0 | 0.2 |
| Average Amount of Any Benefits Received, by Year | | | | | | |
| All years | 3,931.3 | 4,428.0 | -496.8** | 3,770.1 | -699.6** | -15.7 |
| 1 | 830.6 | 1,016.9 | -186.4*** | 752.9 | -262.5*** | -25.8 |
| 2 | 925.9 | 1,081.5 | -155.6** | 852.5 | -219.1** | -20.4 |
| 3 | 1,045.4 | 1,103.9 | -58.5 | 1,003.3 | -82.4 | -7.6 |
| 4 | 1,061.8 | 1,107.3 | -45.4 | 1,067.4 | -64.0 | -5.7 |
| Percentage Received AFDC/TANF Benefits, by Year | | | | | | |
| All years | 41.7 | 39.5 | 2.2 | 41.9 | 3.1 | 8.0 |
| 1 | 22.6 | 22.9 | -0.3 | 22.4 | -0.5 | -2.1 |
| 2 | 19.2 | 21.4 | -2.2 | 18.0 | -3.1 | -14.8 |
| 3 | 20.7 | 20.2 | 0.5 | 20.9 | 0.8 | 3.8 |
| 4 | 18.1 | 16.5 | 1.6 | 19.2 | 2.3 | 13.3 |
| Average Number of Months Ever Received AFDC/TANF Benefits | 5.6 | 6.0 | -0.4 | 5.5 | -0.6 | -9.5 |

TABLE E.3 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Amount of AFDC/TANF Benefits Ever Received (in Dollars) | 1,579.9 | 1,769.1 | -189.2 | 1,566.9 | -266.5 | -14.5 |
| Percentage Received Food Stamp Benefits, by Year | | | | | | |
| All years | 56.2 | 57.2 | -1.0 | 55.8 | -1.4 | -2.5 |
| 1 | 33.4 | 35.1 | -1.7 | 31.9 | -2.5 | -7.1 |
| 2 | 27.1 | 28.2 | -1.2 | 25.5 | -1.6 | -6.0 |
| 3 | 28.5 | 28.7 | -0.2 | 27.9 | -0.3 | -1.1 |
| 4 | 27.9 | 26.0 | 1.9 | 28.1 | 2.7 | 10.6 |
| Average Number of Months Ever Received Food Stamp Benefits | 7.8 | 8.1 | -0.3 | 7.3 | -0.5 | -6.1 |
| Average Amount of Food Stamp Benefits Ever Received (in Dollars) | 1,432.4 | 1,462.4 | -30.1 | 1,350.3 | -42.3 | -3.0 |
| Covered by Public Health Insurance | | | | | | |
| At the 30-month interview | 41.7 | 41.1 | 0.7 | 41.4 | 0.9 | 2.3 |
| At the 48-month interview | 44.0 | 43.7 | 0.3 | 44.2 | 0.4 | 0.8 |
| Percentage Ever Received General Assistance Benefits | 4.3 | 5.4 | -1.1 | 3.4 | -1.5 | -30.6 |
| Average Amount of General Assistance Benefits Ever Received (in Dollars) | 103.4 | 138.1 | -34.7 | 92.9 | -48.9 | -34.5 |
| Percentage Ever Received SSI/SSA Benefits | 9.9 | 12.4 | -2.5** | 9.1 | -3.5** | -27.8 |
| Average Amount of SSI/SSA Benefits Ever Received (in Dollars) | 717.1 | 1,000.9 | -283.8** | 647.7 | -399.6** | -38.2 |
| Percentage Lived in Public Housing | | | | | | |
| At the 30-month interview | 16.3 | 16.9 | -0.6 | 16.4 | -0.8 | -4.6 |
| At the 48-month interview | 17.0 | 18.0 | -1.0 | 17.6 | -1.4 | -7.3 |
| Percentage Ever Received Child Support | 6.2 | 5.3 | 0.9 | 5.9 | 1.2 | 26.1 |
| **Sample Size** | **2,060** | **1,146** | **3,206** | **1,477** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three gender subgroups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

TABLE E.3 *(continued)*

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[c] Estimated impacts per Job Corps participant are measured as the differences between the weighted means for program and control group members divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.4

## IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE FOR FEMALES WITH CHILDREN

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Received Any Benefit (AFDC/TANF, Food Stamps, SSI/SSA, or GA), by Quarter After Random Assignment | | | | | | |
| 1 | 77.7 | 78.7 | -1.0 | 78.0 | -1.6 | -2.0 |
| 2 | 70.9 | 72.6 | -1.7 | 71.9 | -2.7 | -3.6 |
| 3 | 71.3 | 72.4 | -1.2 | 71.8 | -1.9 | -2.5 |
| 4 | 73.1 | 73.5 | -0.5 | 73.5 | -0.8 | -1.0 |
| 5* | 77.5 | 74.6 | 2.9 | 79.6 | 4.6 | 6.2 |
| 6 | 64.6 | 67.8 | -3.2 | 63.8 | -5.1 | -7.4 |
| 7 | 60.1 | 63.7 | -3.6 | 58.3 | -5.7 | -9.0 |
| 8 | 58.5 | 63.7 | -5.2** | 56.5 | -8.4** | -13.0 |
| 9 | 57.5 | 62.8 | -5.4** | 56.3 | -8.6** | -13.3 |
| 10* | 57.6 | 64.3 | -6.7** | 56.3 | -10.7** | -16.0 |
| 11 | 61.6 | 68.1 | -6.5** | 60.8 | -10.5** | -14.7 |
| 12 | 51.9 | 59.0 | -7.2*** | 51.3 | -11.5*** | -18.4 |
| 13 | 50.2 | 55.0 | -4.8* | 49.3 | -7.7* | -13.4 |
| 14 | 48.8 | 53.6 | -4.8* | 48.4 | -7.7* | -13.7 |
| 15 | 48.8 | 51.3 | -2.5 | 49.4 | -4.0 | -7.5 |
| 16 | 49.3 | 53.4 | -4.1 | 48.6 | -6.6 | -12.0 |
| Percentage Received Any Benefits, by Year | | | | | | |
| All years | 92.1 | 93.6 | -1.5 | 91.8 | -2.4 | -2.6 |
| 1 | 82.6 | 84.3 | -1.7 | 81.5 | -2.7 | -3.2 |
| 2 | 82.1 | 82.6 | -0.5 | 83.8 | -0.8 | -0.9 |
| 3 | 68.4 | 73.2 | -4.8* | 67.5 | -7.7* | -10.2 |
| 4 | 59.6 | 62.9 | -3.3 | 58.0 | -5.3 | -8.4 |
| Average Number of Months Received Any Benefits, by Year | | | | | | |
| All years | 27.8 | 29.4 | -1.6* | 27.4 | -2.5* | -8.5 |
| 1 | 8.4 | 8.6 | -0.2 | 8.5 | -0.2 | -2.8 |
| 2 | 7.4 | 7.7 | -0.3 | 7.3 | -0.4 | -5.4 |
| 3 | 6.5 | 7.2 | -0.7** | 6.4 | -1.1** | -15.0 |
| 4 | 5.7 | 6.1 | -0.4 | 5.7 | -0.7 | -10.9 |
| Average Amount of Any Benefits Received, by Year | | | | | | |
| All years | 12,833.1 | 13,402.8 | -569.7 | 12,725.1 | -918.2 | -6.7 |
| 1 | 4,120.9 | 4,105.0 | 15.9 | 4,269.2 | 25.6 | 0.6 |
| 2 | 3,524.8 | 3,665.2 | -140.5 | 3,482.7 | -226.4 | -6.1 |
| 3 | 3,074.5 | 3,354.2 | -279.7 | 2,980.1 | -450.8 | -13.1 |
| 4 | 2,571.6 | 2,783.6 | -212 | 2,618.5 | -341.7 | -11.5 |
| Percentage Received AFDC/TANF Benefits, by Year | | | | | | |
| All years | 80.5 | 81.3 | -0.8 | 81.7 | -1.3 | -1.6 |
| 1 | 68.6 | 70.7 | -2.1 | 69.3 | -3.4 | -4.6 |
| 2 | 66.0 | 67.9 | -1.9 | 68.0 | -3.1 | -4.4 |
| 3 | 51.1 | 54.4 | -3.2 | 50.6 | -5.2 | -9.4 |
| 4 | 35.2 | 39.6 | -4.4* | 35.2 | -7.0* | -16.6 |

TABLE E.4 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Average Number of Months Ever Received AFDC/TANF Benefits | 19.8 | 20.7 | -1.0 | 20.0 | -1.5 | -7.2 |
| Average Amount of AFDC/TANF Benefits Ever Received (in Dollars) | 6,220.4 | 6,471.9 | -251.6 | 6,369.0 | -405.5 | -6.0 |
| Percentage Received Food Stamp Benefits, by Year | | | | | | |
| All years | 88.6 | 89.8 | -1.2 | 88.6 | -1.9 | -2.1 |
| 1 | 77.2 | 78.6 | -1.5 | 75.8 | -2.4 | -3.0 |
| 2 | 75.0 | 76.1 | -1.1 | 75.4 | -1.7 | -2.2 |
| 3 | 61.6 | 65.1 | -3.5 | 60.4 | -5.7 | -8.6 |
| 4 | 52.4 | 55.3 | -2.9 | 50.8 | -4.7 | -8.5 |
| Average Number of Months Ever Received Food Stamp Benefits | 24.2 | 25.5 | -1.3 | 23.4 | -2.1 | -8.3 |
| Average Amount of Food Stamp Benefits Ever Received (in Dollars) | 5,556.4 | 5,790.0 | -233.7 | 5,301.7 | -376.7 | -6.6 |
| Covered by Public Health Insurance | | | | | | |
| At 30-month interview | 70.7 | 73.4 | -2.6 | 69.6 | -4.3 | -5.8 |
| At 48-month interview | 65.8 | 66.0 | -0.3 | 63.1 | -0.4 | -0.7 |
| Percentage Ever Received General Assistance Benefits | 4.9 | 3.8 | 1.2 | 5.5 | 1.9 | 51.4 |
| Average Amount of General Assistance Benefits Ever Received (in Dollars) | 160.9 | 167.2 | -6.3 | 216.5 | -10.1 | -4.5 |
| Percentage Ever Received SSI/SSA Benefits | 11.2 | 12.1 | -1.0 | 12.0 | -1.6 | -11.7 |
| Average Amount of SSI/SSA Benefits Ever Received (in Dollars) | 1,236.0 | 1,357.6 | -121.6 | 1,324.6 | -195.9 | -12.9 |
| Percentage Lived in Public Housing | | | | | | |
| At the 30-month interview | 28.5 | 30.8 | -2.3 | 26.8 | -3.8 | -12.3 |
| At the 48-month interview | 27.7 | 27.8 | -0.1 | 28.6 | -0.1 | -0.4 |
| Percentage Ever Received Child Support | 20.2 | 20.0 | 0.2 | 20.8 | 0.4 | 1.8 |
| **Sample Size** | **1,005** | **538** | **1,543** | **637** | | |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

TABLE E.4 *(continued)*

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NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three gender subgroups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the differences between the weighted means for program and control group members divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.5

## IMPACTS ON THE RECEIPT OF KEY TYPES OF PUBLIC ASSISTANCE, BY RESIDENTIAL DESIGNATION STATUS, AGE, HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE

| | Percentage Received AFDC/TANF Benefits | | Average Amount of AFDC/TANF Benefits Ever Received (in Dollars) | | Percentage Received Food Stamp Benefits | | Average Amount of Food Stamp Benefits Ever Received (in Dollars) | | Percentage Covered by Public Health Insurance at the 48-Month Interview | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] |
| Residential Designees | | | | | | | | | | |
| Males | 20.5 | -2.8** | 535.9 | -172.2*** | 35.3 | -5.9*** | 553.5 | -145.4*** | 24.8 | -3.5** |
| Females without children | 38.8 | 4.0 | 1,735.2 | -231.2 | 56.7 | -1.0 | 1,441.5 | -33.7 | 44.4 | 0.3 |
| Females with children | 78.1 | -3.5 | 4,987.6 | -2.0 | 89.6 | -7.8* | 4,922.9 | -417.4 | 62.3 | 0.8 |
| (P-value)[b] | | .082* | | .927 | | .261 | | .632 | | .387 |
| Nonresidential Designees | | | | | | | | | | |
| Males | 23.2 | 0.0 | 551.1 | 216.6 | 38.3 | -0.1 | 658.3 | 172.6 | 21.7 | 3.1 |
| Females without children | 44.3 | -3.9 | 2,006.2 | -539.3 | 60.6 | -4.4 | 1,608.8 | -102.1 | 39.5 | 0.9 |
| Females with children | 84.2 | 0.8 | 7,794.4 | -773.5 | 89.8 | 3.9 | 6,551.9 | -298.2 | 69.3 | -1.5 |
| (P-value)[b] | | .833 | | .198 | | .517 | | .620 | | .809 |
| Age at Application | | | | | | | | | | |
| 16 and 17 | 34.9 | -1.6 | 1,387.1 | -180.7 | 47.6 | -4.4** | 1,164.4 | -118.5 | 35.7 | -2.4 |
| 18 and 19 | 31.8 | -0.3 | 1,523.0 | -134.8 | 46.7 | -1.4 | 1,359.7 | 67.3 | 34.2 | 1.6 |
| 20 to 24 | 33.3 | 1.5 | 2,055.4 | -221.0 | 51.4 | -5.4* | 2,003.3 | -318.2 | 35.3 | -3.8 |
| (P-value)[b] | | .602 | | .947 | | .485 | | .261 | | .250 |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 29.1 | -0.9 | 1,414.3 | -205.3 | 45.8 | -4.8 | 1,365.3 | -125.1 | 32.2 | -1.4 |
| Had no high school credential | 34.8 | -0.3 | 1,668.5 | -159.3 | 49.2 | -3.3** | 1,475.4 | -94.3 | 36.1 | -1.7 |
| (P-value)[b] | | .844 | | .893 | | .724 | | .906 | | .891 |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 34.3 | -0.1 | 1,705.2 | -157.5 | 49.2 | -3.1** | 1,529.1 | -101.1 | 35.7 | -0.8 |
| Ever arrested | 32.7 | -2.1 | 1,319.7 | -87.1 | 45.9 | -3.1 | 1,200.8 | -35.1 | 32.8 | -2.1 |
| (P-value)[b] | | .528 | | .728 | | .961 | | .690 | | .691 |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 22.7 | -2.7 | 994.1 | -226.0 | 40.7 | -5.4** | 976.7 | 273.7** | 27.4 | -0.3 |
| Black non-Hispanic | 40.2 | -0.5 | 1,930.6 | 168.3 | 52.1 | -1.6 | 1,801.7 | -44.0 | 39.6 | -3.4* |
| Hispanic | 32.2 | 1.3 | 1,618.8 | 2.0 | 49.6 | -4.2 | 1,265.9 | 32.5 | 34.5 | 1.9 |
| Other | 32.1 | 6.1 | 1,707.5 | -268.2 | 48.7 | -8.0 | 1,307.9 | -155.4 | 35.5 | -0.7 |
| (P-value)[b] | | .358 | | .885 | | .503 | | .375 | | .451 |

TABLE E.5 *(continued)*

| | Percentage Received AFDC/TANF Benefits | | Average Amount of AFDC/TANF Benefits Ever Received (in Dollars) | | Percentage Received Food Stamp Benefits | | Average Amount of Food Stamp Benefits Ever Received (in Dollars) | | Percentage Covered by Public Health Insurance at the 48-Month Interview | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 34.1 | -0.3 | 1,751.1 | -549.9*** | 48.3 | -4.6 | 1,554.0 | -391.6** | 35.8 | -2.9 |
| On or after 3/1/95 (after ZT) | 33.3 | -0.5 | 1,566.8 | -60.5 | 48.4 | -3.4** | 1,418.8 | -23.4 | 34.9 | -1.2 |
| (P-value)[b] | | .962 | | .038** | | .741 | | .048** | | .592 |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per program participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[b] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[c] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# APPENDIX F

## SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON CRIME-RELATED OUTCOMES

## TABLE F.1

## IMPACTS ON FINER CATEGORIES OF ARREST CHARGES

| Category | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Murder | 0.4 | 0.5 | 0.0 | 0.4 | 0.0 | -9.7 |
| Aggravated Assault | 3.9 | 3.7 | 0.2 | 3.8 | 0.3 | 7.5 |
| Robbery | 2.1 | 2.2 | -0.1 | 1.9 | -0.2 | -8.1 |
| Burglary | 2.7 | 3.0 | -0.4 | 2.3 | -0.5 | -17.9 |
| Larceny, Theft, and Other Property Crimes (Percentage with Charge) | | | | | | |
| Forgery or counterfeiting | 0.6 | 1.0 | -0.4** | 0.5 | -0.6** | -52.0 |
| Larceny/theft | 2.6 | 2.9 | -0.2 | 2.4 | -0.3 | -12.1 |
| Motor vehicle theft/carjacking | 1.5 | 1.7 | -0.2 | 1.4 | -0.2 | -15.3 |
| Shoplifting | 1.1 | 1.1 | -0.1 | 1.0 | -0.1 | -6.5 |
| Buying/receiving/possessing stolen property | 1.6 | 1.4 | 0.2 | 1.5 | 0.2 | 17.1 |
| Vandalism | 1.0 | 1.0 | 0.0 | 1.1 | 0.0 | 3.9 |
| Bad checks | 0.5 | 0.4 | 0.1 | 0.5 | 0.1 | 34.8 |
| Drug-Law Violations (Percentage with Charge) | | | | | | |
| Use or possession of drugs or drug equipment | 5.9 | 6.9 | -1.0** | 5.6 | -1.4** | -20.4 |
| Sale or manufacture of drugs | 2.6 | 2.7 | -0.2 | 2.4 | -0.2 | -8.4 |
| Other Personal Crimes (Percentage with Charge) | | | | | | |
| Simple assault | 3.7 | 3.7 | 0.0 | 3.8 | 0.0 | -0.5 |
| Family offenses | 0.7 | 0.7 | 0.0 | 0.5 | 0.1 | 11.1 |
| Sex offenses other than rape | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 0.4 |
| Fighting | 0.5 | 0.8 | -0.2 | 0.6 | -0.3 | -33.9 |
| Miscellaneous Crimes (Percentage with Charge) | | | | | | |
| Disorderly conduct | 3.3 | 3.7 | -0.4 | 3.1 | -0.6 | -15.7 |
| Liquor-related crimes | 3.6 | 4.7 | -1.1*** | 3.6 | -1.5*** | -28.7 |
| Loitering or vagrancy or curfew violations | 0.8 | 1.2 | -0.4** | 0.9 | -0.5** | -36.7 |
| Parole or probation violations | 3.7 | 4.2 | -0.4 | 2.9 | -0.6 | -16.6 |
| Weapons offenses | 2.7 | 2.4 | 0.3 | 2.6 | 0.4 | 16.1 |
| Trespassing | 1.8 | 2.0 | -0.2 | 1.7 | -0.3 | -13.7 |
| Having an outstanding warrant | 1.0 | 1.4 | -0.4* | 1.0 | -0.5* | -33.7 |
| Obstruction of justice | 2.8 | 3.2 | -0.5 | 2.6 | -0.6 | -20.0 |
| Other motor vehicle violations | 3.7 | 4.7 | -1.0** | 3.6 | -1.3 | -27.5 |
| Smoking cigarettes under age | 0.9 | 1.2 | -0.4** | 0.8 | -0.5 | 38.9 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

TABLE F.1 *(continued)*

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SOURCE: 12-, 30-, and 48-month interview data for those who completed 48-month interviews.

NOTES: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

Impact estimates are presented only for crimes committed by at least 15 program group members and 15 control group members.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.2

## IMPACTS ON THE NUMBER OF ARREST CHARGES, BY YEAR

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Number of Times Charged, All Years | | | | | | |
| Murder | 0.004 | 0.005 | -0.001 | 0.004 | -0.001 | -15.1 |
| Assault | 0.041 | 0.038 | 0.003 | 0.041 | 0.004 | 11.8 |
| Robbery | 0.022 | 0.023 | -0.001 | 0.019 | -0.002 | -7.6 |
| Burglary | 0.030 | 0.035 | -0.005 | 0.025 | -0.008 | -23.6 |
| Larceny, vehicle theft, or other property crimes | 0.109 | 0.113 | -0.004 | 0.104 | -0.005 | -4.5 |
| Drug law violations | 0.105 | 0.118 | -0.013 | 0.102 | -0.018 | -15.0 |
| Other personal crimes | 0.051 | 0.054 | -0.003 | 0.051 | -0.005 | -8.1 |
| Other miscellaneous crimes | 0.287 | 0.353 | -0.066*** | 0.270 | -0.092*** | -25.4 |
| Average Number of Times Charged, Year 1 | | | | | | |
| Murder | 0.001 | 0.002 | -0.001 | 0.001 | -0.001 | -42.8 |
| Assault | 0.012 | 0.013 | 0.000 | 0.012 | 0.000 | -0.3 |
| Robbery | 0.009 | 0.009 | 0.000 | 0.008 | -0.001 | -7.9 |
| Burglary | 0.010 | 0.016 | -0.006** | 0.008 | -0.008** | -49.3 |
| Larceny, vehicle theft, or other property crimes | 0.031 | 0.040 | -0.008** | 0.028 | -0.012** | -29.9 |
| Drug law violations | 0.018 | 0.028 | -0.009*** | 0.015 | -0.013*** | -46.1 |
| Other personal crimes | 0.014 | 0.017 | -0.003 | 0.013 | -0.004 | -24.3 |
| Other miscellaneous crimes | 0.071 | 0.100 | -0.030*** | 0.056 | -0.041*** | -42.5 |
| Average Number of Times Charged, Year 2 | | | | | | |
| Murder | 0.001 | 0.001 | -0.001 | 0.001 | -0.001 | -45.8 |
| Assault | 0.009 | 0.008 | 0.001 | 0.009 | 0.001 | 17.6 |
| Robbery | 0.006 | 0.008 | -0.002 | 0.005 | -0.003 | -36.5 |
| Burglary | 0.009 | 0.006 | 0.003 | 0.007 | 0.004 | 128.6 |
| Larceny, vehicle theft, or other property crimes | 0.031 | 0.027 | 0.004 | 0.028 | 0.006 | 27.0 |
| Drug law violations | 0.027 | 0.030 | -0.003 | 0.026 | -0.004 | -13.4 |
| Other personal crimes | 0.013 | 0.011 | 0.002 | 0.015 | 0.003 | 25.4 |
| Other miscellaneous crimes | 0.063 | 0.081 | -0.018*** | 0.064 | -0.025*** | -27.8 |
| Average Number of Times Charged, Year 3 | | | | | | |
| Murder | 0.001 | 0.001 | 0.001 | 0.001 | 0.001 | 147.8 |
| Assault | 0.012 | 0.007 | 0.005*** | 0.013 | 0.007*** | 127.8 |
| Robbery | 0.003 | 0.003 | 0.000 | 0.002 | 0.000 | -9.2 |
| Burglary | 0.006 | 0.007 | -0.001 | 0.005 | -0.002 | -25.[illegible] |
| Larceny, vehicle theft, or other property crimes | 0.025 | 0.024 | 0.000 | 0.024 | 0.000 | 2.0 |
| Drug law violations | 0.033 | 0.032 | 0.001 | 0.032 | 0.001 | 3.1 |
| Other personal crimes | 0.014 | 0.015 | -0.001 | 0.013 | -0.001 | -5.7 |
| Other miscellaneous crimes | 0.080 | 0.089 | -0.010 | 0.079 | -0.014 | -14.7 |

TABLE F.2 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Number of Times Charged, Year 4 | | | | | | |
| Murder | 0.001 | 0.001 | 0.000 | 0.001 | 0.000 | -8.0 |
| Assault | 0.007 | 0.010 | -0.003 | 0.008 | -0.004 | -33.4 |
| Robbery | 0.004 | 0.002 | 0.002 | 0.004 | 0.002 | 112.7 |
| Burglary | 0.005 | 0.006 | -0.001 | 0.005 | -0.002 | -30.9 |
| Larceny, vehicle theft, or other property crimes | 0.023 | 0.022 | 0.000 | 0.024 | 0.001 | 2.9 |
| Drug law violations | 0.028 | 0.029 | -0.001 | 0.029 | -0.002 | -6.0 |
| Other personal crimes | 0.010 | 0.012 | -0.002 | 0.011 | -0.003 | -19.8 |
| Other miscellaneous crimes | 0.073 | 0.082 | -0.009 | 0.071 | -0.012 | -14.8 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.3

## IMPACTS ON KEY CRIME OUTCOMES FOR 16- AND 17-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1* | 3.0 | 4.3 | -1.4** | 2.4 | -1.8** | -42.1 |
| 2* | 3.4 | 5.0 | -1.6*** | 3.1 | -2.1*** | -40.4 |
| 3 | 5.1 | 5.9 | -0.8 | 4.4 | -1.0 | -18.8 |
| 4 | 6.2 | 6.9 | -0.6 | 5.5 | -0.8 | -12.8 |
| 5 | 4.5 | 4.9 | -0.4 | 3.7 | -0.5 | -12.4 |
| 6 | 3.2 | 4.1 | -0.9 | 3.1 | -1.1 | -26.3 |
| 7 | 3.6 | 4.2 | -0.6 | 3.8 | -0.8 | -16.9 |
| 8 | 4.1 | 4.7 | -0.6 | 4.0 | -0.8 | -16.2 |
| 9 | 5.1 | 4.7 | 0.4 | 5.3 | 0.5 | 10.0 |
| 10 | 5.1 | 5.2 | -0.1 | 4.9 | -0.1 | -2.1 |
| 11 | 5.2 | 4.9 | 0.2 | 4.4 | 0.3 | 7.0 |
| 12 | 3.2 | 3.2 | 0.0 | 2.9 | 0.0 | 0.3 |
| 13 | 3.7 | 3.7 | 0.0 | 3.8 | 0.0 | -1.1 |
| 14 | 3.8 | 4.3 | -0.5 | 3.8 | -0.6 | -14 |
| 15 | 3.7 | 3.8 | -0.1 | 3.5 | -0.1 | -4.0 |
| 16 | 5.4 | 6.2 | -0.8 | 5.6 | -1.0 | -14.8 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years | 38.1 | 41.4 | -3.4** | 36.0 | -4.3** | -10.8 |
| 1 | 15.4 | 18.3 | -2.9*** | 13.5 | -3.8*** | -21.9 |
| 2 | 13.6 | 15.2 | -1.6 | 12.9 | -2.1 | -14.1 |
| 3 | 15.7 | 15.3 | 0.4 | 14.9 | 0.5 | 3.3 |
| 4 | 13.5 | 14.7 | -1.3 | 13.5 | -1.6 | -10.8 |
| Average Number of Times Ever Arrested | 0.9 | 1.0 | -0.1** | 0.9 | -0.1** | -12.8 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder** | 0.6 | 0.6 | 0.0 | 0.6 | 0.0 | -6.7 |
| Assault | 5.7 | 5.8 | 0.0 | 5.4 | 0.0 | -0.8 |
| Robbery* | 3.2 | 3.8 | -0.5 | 2.7 | -0.7 | -20.4 |
| Burglary | 3.9 | 4.8 | -0.9 | 3.3 | -1.2 | -26.0 |
| Larceny, vehicle theft, or other property crimes | 11.5 | 12.4 | -0.9 | 11.0 | -1.1 | -9.2 |
| Drug law violations | 10.1 | 11.4 | -1.2 | 10.0 | -1.6 | -13.8 |
| Other personal crimes | 6.8 | 6.5 | 0.2 | 6.9 | 0.3 | 4.5 |
| Other miscellaneous crimes | 21.9 | 25.0 | -3.1** | 20.6 | -3.9** | -16.1 |
| Percentage Had a Serious Arrest Charge[e] | 11.4 | 12.9 | -1.5 | 10.3 | -2.0 | -16.1 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment | 29.3 | 32.4 | -3.1** | 27.4 | -4.0** | -12.9 |

TABLE F.3 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Made a Deal or Plea-Bargained | 16.3 | 19.4 | -3.1*** | 14.7 | -4.0*** | -21.3 |
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 14.1 |
| Assault | 3.3 | 3.4 | 0.0 | 2.7 | -0.1 | -2.2 |
| Robbery*** | 1.9 | 3.1 | -1.2*** | 1.3 | -1.6*** | -54.6 |
| Burglary | 2.5 | 3.1 | -0.5 | 2.4 | -0.7 | -22.5 |
| Larceny, vehicle theft, or other property crimes | 8.7 | 8.6 | 0.0 | 8.2 | 0.0 | 0.4 |
| Drug law violations | 7.8 | 8.7 | -0.9 | 7.2 | -1.1 | -13.4 |
| Other personal crimes | 4.0 | 4.1 | 0.0 | 4.2 | 0.0 | -0.3 |
| Other miscellaneous crimes | 14.1 | 16.6 | -2.4** | 13.5 | -3.1** | -18.9 |
| Percentage Ever Served Time in Jail for Convictions | 20.7 | 24.2 | -3.5*** | 18.9 | -4.5*** | -19.2 |
| Average Weeks in Jail for Convictions | 8.0 | 8.8 | -0.8 | 6.6 | -1.0 | -13.7 |
| Percentage Ever Put on Probation or Parole | 18.3 | 19.6 | -1.3 | 16.9 | -1.7 | -8.9 |
| **Sample Size** | **2,742** | **1,907** | **4,649** | **2,132** | | |

SOURCE: 12-, 30- and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three age groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.4

## IMPACTS ON KEY CRIME OUTCOMES FOR 18- AND 19-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1* | 1.7 | 3.6 | -1.9*** | 0.8 | -2.7*** | -77.4 |
| 2* | 3.1 | 2.7 | 0.3 | 2.5 | 0.5 | 24.9 |
| 3 | 2.8 | 4.5 | -1.6*** | 3.0 | -2.4*** | -44.5 |
| 4 | 3.5 | 4.5 | -1.0 | 2.9 | -1.4 | -32.6 |
| 5 | 3.7 | 3.2 | 0.4 | 3.5 | 0.6 | 21.0 |
| 6 | 2.3 | 2.5 | -0.2 | 2.0 | -0.3 | -12.1 |
| 7 | 2.1 | 3.1 | -1.0* | 1.9 | -1.4* | -42.8 |
| 8 | 2.5 | 2.6 | -0.1 | 2.4 | -0.2 | -8.3 |
| 9 | 2.3 | 3.1 | -0.7 | 2.2 | -1.1 | -32.4 |
| 10 | 3.1 | 4.4 | -1.3** | 3.2 | -1.9** | -36.9 |
| 11 | 2.9 | 2.1 | 0.8 | 2.7 | 1.2 | 79.4 |
| 12 | 2.4 | 2.1 | 0.2 | 2.5 | 0.3 | 15.9 |
| 13 | 2.1 | 2.3 | -0.3 | 1.9 | -0.4 | -17.1 |
| 14 | 1.8 | 2.1 | -0.3 | 1.4 | -0.4 | -24.4 |
| 15 | 2.5 | 2.5 | 0.0 | 2.3 | 0.0 | 1.3 |
| 16 | 3.0 | 3.7 | -0.7 | 2.8 | -1.0 | -27.6 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years | 25.5 | 30.1 | -4.7*** | 24.3 | -6.7*** | -21.7 |
| 1 | 9.6 | 12.9 | -3.2*** | 8.4 | -4.7*** | -35.8 |
| 2 | 9.1 | 9.6 | -0.6 | 8.3 | -0.8 | -9.1 |
| 3 | 9.4 | 10.1 | -0.7 | 9.0 | -1.0 | -9.6 |
| 4 | 8.3 | 8.9 | -0.6 | 7.4 | -0.8 | -10.2 |
| Average Number of Times Ever Arrested | 0.6 | 0.7 | -0.1** | 0.5 | -0.1** | -21.3 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder** | 0.3 | 0.6 | -0.3 | 0.2 | -0.5 | -69.4 |
| Assault | 3.5 | 2.8 | 0.7 | 3.9 | 1.1 | 38.7 |
| Robbery* | 1.5 | 1.8 | -0.2 | 1.6 | -0.3 | -16.9 |
| Burglary | 2.5 | 2.1 | 0.3 | 2.0 | 0.5 | 33.0 |
| Larceny, vehicle theft, or other property crimes | 6.3 | 7.1 | -0.9 | 5.1 | -1.2 | -19.4 |
| Drug law violations | 6.1 | 5.9 | 0.2 | 5.1 | 0.3 | 6.7 |
| Other personal crimes | 4.2 | 4.7 | -0.6 | 4.2 | -0.8 | -16.4 |
| Other miscellaneous crimes | 14.8 | 18.3 | -3.5*** | 13.6 | -5.1*** | -27.1 |
| Percentage Had a Serious Arrest Charge[e] | 6.6 | 6.6 | 0.0 | 6.6 | 0.0 | 0.0 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment | 19.3 | 23.0 | -3.7*** | 17.9 | -5.3*** | -23.0 |

TABLE F.4 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Made a Deal or Plea-Bargained | 10.8 | 12.3 | -1.5 | 9.2 | -2.2 | -19.0 |
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.2 | 0.3 | -0.2 | 0.2 | -0.2 | -52.8 |
| Assault | 1.9 | 1.8 | 0.1 | 2.1 | 0.2 | 11.3 |
| Robbery*** | 1.2 | 1.4 | -0.1 | 0.9 | -0.2 | -18.2 |
| Burglary | 1.6 | 1.4 | 0.2 | 1.4 | 0.3 | 29.0 |
| Larceny, vehicle theft, or other property crimes | 4.5 | 5.0 | -0.6 | 3.7 | -0.8 | -18.1 |
| Drug law violations | 4.6 | 4.5 | 0.1 | 3.9 | 0.1 | 2.7 |
| Other personal crimes | 2.8 | 3.0 | -0.3 | 3.0 | -0.4 | -11.6 |
| Other miscellaneous crimes | 10.5 | 12.1 | -1.6 | 9.2 | -2.4 | -20.7 |
| Percentage Ever Served Time in Jail for Convictions | 14.5 | 15.5 | -1.0 | 12.8 | -1.4 | -10.1 |
| Average Weeks in Jail for Convictions | 5.3 | 6.1 | -0.9 | 4.4 | -1.2 | -21.9 |
| Percentage Ever Put on Probation or Parole | 11.1 | 12.8 | -1.6 | 10.1 | -2.3 | -18.8 |
| **Sample Size** | **2,175** | **1,402** | **3,577** | **1,518** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three age groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE F.5

## IMPACTS ON KEY CRIME OUTCOMES FOR 20- TO 24-YEAR-OLDS

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1* | 1.9 | 2.2 | -0.3 | 1.0 | -0.4 | -30.4 |
| 2* | 1.5 | 2.1 | -0.6 | 1.0 | -0.8 | -47.1 |
| 3 | 1.7 | 2.6 | -0.9 | 1.2 | -1.3 | -51.3 |
| 4 | 1.9 | 3.5 | -1.6*** | 1.4 | -2.4*** | -62.9 |
| 5 | 2.5 | 2.2 | 0.2 | 2.2 | 0.4 | 19.8 |
| 6 | 2.1 | 1.9 | 0.2 | 2.0 | 0.2 | 14.0 |
| 7 | 2.0 | 1.9 | 0.1 | 1.8 | 0.1 | 5.0 |
| 8 | 1.6 | 2.0 | -0.3 | 1.9 | -0.5 | -20.2 |
| 9 | 1.6 | 2.2 | -0.6 | 1.8 | -0.9 | -33.8 |
| 10 | 2.2 | 2.3 | -0.1 | 2.2 | -0.1 | -5.4 |
| 11 | 1.6 | 2.4 | -0.7 | 1.0 | -1.1 | -51.8 |
| 12 | 1.4 | 1.1 | 0.3 | 1.5 | 0.5 | 48.5 |
| 13 | 1.3 | 1.6 | -0.3 | 1.4 | -0.4 | -22.8 |
| 14 | 1.2 | 0.6 | 0.6 | 1.3 | 0.9 | 215.6 |
| 15 | 1.5 | 1.5 | -0.1 | 1.6 | -0.1 | -5.6 |
| 16 | 1.9 | 2.1 | -0.2 | 2.0 | -0.3 | -13.0 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years | 18.7 | 21.7 | -3.0** | 16.7 | -4.5** | -21.3 |
| 1 | 6.2 | 9.2 | -3.0*** | 4.1 | -4.5*** | -52.2 |
| 2 | 7.4 | 7.3 | 0.1 | 7.0 | 0.2 | 2.3 |
| 3 | 6.0 | 7.1 | -1.1 | 5.3 | -1.7 | -24.5 |
| 4 | 5.4 | 5.2 | 0.2 | 5.7 | 0.3 | 5.4 |
| Average Number of Times Ever Arrested | 0.4 | 0.4 | -0.1* | 0.3 | -0.1* | -22.0 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder** | 0.3 | 0.0 | 0.3** | 0.3 | 0.5** | -196.5 |
| Assault | 2.3 | 2.1 | 0.1 | 2.0 | 0.2 | 10.9 |
| Robbery* | 1.0 | 0.3 | 0.7** | 0.8 | 1.0** | -460.7 |
| Burglary | 1.0 | 1.3 | -0.3 | 0.8 | -0.5 | -39.1 |
| Larceny, vehicle theft, or other property crimes | 4.6 | 4.6 | 0.0 | 4.3 | 0.0 | 1.0 |
| Drug law violations | 3.9 | 4.9 | -1.0 | 3.0 | -1.5 | -34.0 |
| Other personal crimes | 4.2 | 4.6 | -0.4 | 3.9 | -0.6 | -14.2 |
| Other miscellaneous crimes | 10.7 | 12.4 | -1.6 | 9.2 | -2.4 | -21.0 |
| Percentage Had a Serious Arrest Charge[e] | 4.2 | 3.5 | 0.8 | 3.3 | 1.1 | 52.8 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment | 14.4 | 16.6 | -2.1 | 12.8 | -3.2 | -19.9 |
| Percentage Made a Deal or Plea-Bargained | 7.8 | 8.9 | -1.1 | 7.2 | -1.7 | -19.4 |

TABLE F.5 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.2 | 0.0 | 0.2* | 0.1 | 0.3* | -132.5 |
| Assault | 1.4 | 1.0 | 0.3 | 1.2 | 0.5 | 72.5 |
| Robbery*** | 0.7 | 0.2 | 0.5* | 0.4 | 0.7* | -259.9 |
| Burglary | 0.6 | 1.3 | -0.7** | 0.5 | -1.1** | -68.3 |
| Larceny, vehicle theft, or other property crimes | 3.2 | 3.3 | -0.1 | 3.4 | -0.1 | -3.9 |
| Drug law violations | 3.2 | 3.7 | -0.5 | 2.2 | -0.7 | -24.3 |
| Other personal crimes | 2.1 | 2.8 | -0.6 | 2 | -0.9 | -31.5 |
| Other miscellaneous crimes | 7.8 | 8.4 | -0.6 | 6.7 | -0.9 | -11.9 |
| Percentage Ever Served Time in Jail for Convictions | 10.0 | 11.3 | -1.2 | 9.3 | -1.9 | -16.8 |
| Average Weeks in Jail for Convictions | 3.7 | 3.6 | 0.2 | 2.9 | 0.2 | 9.0 |
| Percentage Ever Put on Probation or Parole | 8.8 | 9.2 | -0.4 | 7.8 | -0.6 | -7.0 |
| **Sample Size** | **1,911** | **1,176** | **3,087** | **1,275** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the three age groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.6

## IMPACTS ON KEY CRIME OUTCOMES FOR MALES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1 | 3.3 | 4.9 | -1.5*** | 2.3 | -2.1*** | -47.7 |
| 2 | 3.8 | 4.9 | -1.1** | 3.2 | -1.5** | -31.6 |
| 3 | 4.9 | 6.2 | -1.4** | 4.3 | -1.8** | -30.0 |
| 4* | 6.1 | 7.6 | -1.6** | 5.4 | -2.1** | -27.9 |
| 5 | 5.0 | 4.9 | 0.2 | 4.4 | 0.2 | 5.3 |
| 6 | 3.8 | 3.9 | -0.1 | 3.6 | -0.1 | -2.2 |
| 7 | 4.0 | 4.5 | -0.6 | 3.9 | -0.7 | -16.1 |
| 8 | 4.2 | 4.7 | -0.6 | 4.2 | -0.8 | -15.3 |
| 9* | 4.8 | 4.5 | 0.3 | 5.0 | 0.4 | 7.9 |
| 10 | 5.2 | 5.8 | -0.6 | 5.2 | -0.8 | -13.8 |
| 11 | 5.0 | 4.8 | 0.2 | 4.2 | 0.2 | 6.0 |
| 12 | 3.6 | 3.2 | 0.4 | 3.4 | 0.6 | 20.4 |
| 13 | 3.5 | 3.8 | -0.3 | 3.6 | -0.4 | -10.9 |
| 14 | 3.4 | 3.9 | -0.5 | 3.4 | -0.6 | -15.6 |
| 15 | 3.9 | 3.7 | 0.2 | 3.8 | 0.2 | 6.0 |
| 16 | 5.1 | 6.0 | -0.9 | 5.4 | -1.2 | -18.7 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years** | 38.5 | 43.5 | -5.1*** | 36.4 | -6.8*** | -15.7 |
| 1** | 15.5 | 19.7 | -4.2*** | 13.2 | -5.6*** | -29.8 |
| 2 | 14.8 | 15.4 | -0.7 | 13.8 | -0.9 | -6.1 |
| 3 | 15.7 | 15.7 | -0.1 | 14.7 | -0.1 | -0.5 |
| 4 | 13.3 | 14.6 | -1.2 | 13.5 | -1.7 | -11.0 |
| Average Number of Times Ever Arrested | 1.0 | 1.1 | -0.1** | 0.9 | -0.1** | -12.2 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.7 | 0.7 | -0.1 | 0.6 | -0.1 | -17.1 |
| Assault | 5.6 | 4.9 | 0.6 | 5.3 | 0.8 | 18.9 |
| Robbery | 3.3 | 3.6 | -0.2 | 3.0 | -0.3 | -9.4 |
| Burglary | 4.2 | 4.8 | -0.5 | 3.6 | -0.7 | -16.8 |
| Larceny, vehicle theft, or other property crimes* | 10.8 | 10.7 | 0.2 | 10.1 | 0.2 | 2.4 |
| Drug law violations* | 10.7 | 12.0 | -1.3* | 10.0 | -1.8* | -15.3 |
| Other personal crimes | 6.8 | 6.9 | 0.0 | 6.9 | 0.0 | -0.4 |
| Other miscellaneous crimes | 23.2 | 26.9 | -3.6*** | 21.3 | -4.9*** | -18.7 |
| Percentage Had a Serious Arrest Charge[e] | 11.6 | 12.2 | -0.6 | 10.6 | -0.8 | -7.3 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment* | 30.9 | 34.9 | -4.0*** | 28.9 | -5.3*** | -15.6 |
| Percentage Made a Deal or Plea-Bargained*** | 17.9 | 21.3 | -3.4*** | 15.8 | -4.6*** | -22.5 |

TABLE F.6 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 12.9 |
| Assault | 3.4 | 3.0 | 0.4 | 3.0 | 0.5 | 22.1 |
| Robbery | 2.2 | 2.8 | -0.6 | 1.6 | -0.8 | -32.9 |
| Burglary | 2.8 | 3.3 | -0.5 | 2.6 | -0.7 | -21.1 |
| Larceny, vehicle theft, or other property crimes | 7.8 | 7.7 | 0.1 | 7.4 | 0.2 | 2.2 |
| Drug law violations* | 8.3 | 9.3 | -1.0 | 7.5 | -1.4 | -15.7 |
| Other personal crimes | 4.3 | 4.4 | -0.1 | 4.5 | -0.1 | -2.8 |
| Other miscellaneous crimes | 16.6 | 18.5 | -1.9** | 15.2 | -2.6** | -14.5 |
| Percentage Ever Served Time in Jail for Convictions* | 22.9 | 26.0 | -3.0*** | 20.8 | -4.1*** | -16.3 |
| Average Weeks in Jail for Convictions | 9.5 | 10.6 | -1.0 | 7.8 | -1.4 | -15.2 |
| Percentage Ever Put on Probation or Parole | 19.0 | 20.3 | -1.3 | 17.7 | -1.8 | -9.1 |
| **Sample Size** | **3,741** | **2,787** | **6,528** | **2,799** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two gender groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.7

## IMPACTS ON KEY CRIME OUTCOMES FOR FEMALES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1 | 0.8 | 1.6 | -0.8*** | 0.5 | -1.2*** | -69.8 |
| 2 | 1.3 | 1.4 | -0.2 | 1.1 | -0.2 | -16.6 |
| 3 | 1.4 | 2.1 | -0.6* | 1.4 | -0.9* | -39.9 |
| 4* | 1.5 | 1.7 | -0.1 | 1.0 | -0.2 | -17.4 |
| 5 | 1.7 | 1.9 | -0.1 | 1.5 | -0.2 | -11.1 |
| 6 | 0.8 | 1.6 | -0.8** | 0.8 | -1.2** | -58.5 |
| 7 | 0.9 | 1.4 | -0.5 | 0.9 | -0.7 | -45.3 |
| 8 | 1.1 | 1.2 | -0.1 | 1.1 | -0.1 | -10.6 |
| 9* | 1.1 | 2.0 | -1.0*** | 1.1 | -1.4*** | -56.1 |
| 10 | 1.5 | 1.7 | -0.2 | 1.4 | -0.3 | -19.4 |
| 11 | 1.4 | 1.2 | 0.2 | 1.2 | 0.3 | 27.0 |
| 12 | 0.9 | 1.1 | -0.2 | 0.9 | -0.3 | -23.3 |
| 13 | 1.2 | 1.1 | 0.1 | 1.0 | 0.1 | 11.9 |
| 14 | 1.1 | 0.7 | 0.4 | 0.8 | 0.5 | 200.5 |
| 15 | 1.0 | 1.3 | -0.3 | 0.8 | -0.5 | -37.9 |
| 16 | 1.7 | 1.8 | -0.1 | 1.4 | -0.2 | -10.2 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years** | 15.0 | 16.5 | -1.5 | 13.7 | -2.2 | -13.6 |
| 1** | 4.7 | 6.0 | -1.3** | 3.9 | -1.9** | -33.1 |
| 2 | 4.3 | 5.2 | -1.0 | 4.1 | -1.4 | -25.9 |
| 3 | 4.5 | 5.2 | -0.7 | 4.4 | -1.0 | -19.0 |
| 4 | 4.3 | 4.1 | 0.2 | 3.8 | 0.3 | 9.8 |
| Average Number of Times Ever Arrested | 0.2 | 0.3 | -0.1*** | 0.2 | -0.1*** | -34.5 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.1 | 0.0 | 0.1 | 0.1 | 0.1 | 570.1 |
| Assault | 2.0 | 2.2 | -0.2 | 2.2 | -0.4 | -13.9 |
| Robbery | 0.3 | 0.2 | 0.1 | 0.2 | 0.1 | 117.8 |
| Burglary | 0.4 | 0.5 | 0.0 | 0.2 | -0.1 | -20.5 |
| Larceny, vehicle theft, or other property crimes* | 3.9 | 5.6 | -1.7*** | 3.5 | -2.5*** | -41.5 |
| Drug law violations* | 2.1 | 1.8 | 0.3 | 1.7 | 0.4 | 33.9 |
| Other personal crimes | 3.0 | 3.4 | -0.4 | 2.9 | -0.6 | -17.8 |
| Other miscellaneous crimes | 7.2 | 8.6 | -1.5* | 6.6 | -2.2* | -24.5 |
| Percentage Had a Serious Arrest Charge[e] | 2.7 | 2.8 | -0.1 | 2.5 | -0.1 | -4.2 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment* | 9.4 | 11.0 | -1.6* | 8.2 | -2.3* | -21.7 |
| Percentage Made a Deal or Plea-Bargained*** | 4.2 | 4.1 | 0.1 | 3.8 | 0.1 | 3.1 |

TABLE F.7 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.1 | 0.0 | 0.0 | 0.1 | 0.0 | 227.2 |
| Assault | 0.9 | 1.1 | -0.3 | 0.8 | -0.4 | -34.4 |
| Robbery | 0.2 | 0.4 | -0.2 | 0.1 | -0.3 | -84.1 |
| Burglary | 0.2 | 0.3 | -0.1 | 0.1 | -0.1 | -52.5 |
| Larceny, vehicle theft, or other property crimes | 3.0 | 3.6 | -0.6 | 2.7 | -0.9 | -24.7 |
| Drug law violations* | 1.7 | 1.2 | 0.4 | 1.0 | 0.6 | 161.6 |
| Other personal crimes | 1.5 | 1.9 | -0.5 | 1.4 | -0.7 | -32.3 |
| Other miscellaneous crimes | 3.7 | 4.9 | -1.3** | 3.0 | -1.9** | -38.0 |
| Percentage Ever Served in Jail for Convictions* | 5.6 | 6.2 | -0.6 | 4.8 | -0.9 | -15.7 |
| Average Weeks in Jail for Convictions | 0.9 | 0.7 | 0.2 | 0.6 | 0.3 | 83.1 |
| Percentage Ever Put on Probation or Parole | 5.5 | 6.3 | -0.9 | 4.4 | -1.2 | -21.9 |
| **Sample Size** | **3,087** | **1,698** | **4,785** | **2,126** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two gender groups.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.8

## IMPACTS ON KEY CRIME OUTCOMES FOR MALE RESIDENTIAL DESIGNEES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1 | 3.3 | 5.1 | -1.8*** | 2.2 | -2.4*** | -52.3 |
| 2** | 3.8 | 5.1 | -1.2** | 3.1 | -1.6** | -34.0 |
| 3 | 5.0 | 6.4 | -1.4** | 4.4 | -1.9** | -30.1 |
| 4** | 6.2 | 7.9 | -1.7*** | 5.6 | -2.3*** | -29.4 |
| 5 | 5.2 | 5.0 | 0.2 | 4.5 | 0.2 | 5.0 |
| 6 | 3.9 | 4.0 | -0.1 | 3.5 | -0.2 | -4.4 |
| 7 | 3.9 | 4.5 | -0.6 | 3.9 | -0.8 | -16.9 |
| 8 | 4.3 | 4.8 | -0.5 | 4.3 | -0.7 | -13.2 |
| 9** | 4.9 | 4.7 | 0.2 | 5.0 | 0.2 | 4.3 |
| 10 | 5.2 | 5.9 | -0.7 | 5.1 | -0.9 | -15.6 |
| 11 | 4.9 | 4.8 | 0.1 | 4.3 | 0.2 | 4.5 |
| 12 | 3.7 | 3.1 | 0.5 | 3.4 | 0.7 | 26.4 |
| 13 | 3.6 | 3.9 | -0.3 | 3.7 | -0.4 | -9.8 |
| 14** | 3.3 | 4.1 | -0.7 | 3.4 | -1.0 | -21.9 |
| 15 | 3.9 | 3.9 | 0.1 | 3.9 | 0.1 | 2.3 |
| 16 | 5.1 | 6.1 | -1.0* | 5.4 | -1.4* | -19.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years*** | 38.6 | 44.3 | -5.8*** | 36.4 | -7.7*** | -17.5 |
| 1*** | 15.6 | 20.3 | -4.6*** | 13.3 | -6.2*** | -31.8 |
| 2 | 14.9 | 15.6 | -0.7 | 14.0 | -1.0 | -6.4 |
| 3 | 15.8 | 16.0 | -0.2 | 14.7 | -0.3 | -1.9 |
| 4** | 13.4 | 15.0 | -1.5* | 13.7 | -2.1* | -13.1 |
| Average Number of Times Ever Arrested | 1.0 | 1.1 | -0.1** | 0.9 | -0.1** | -14.0 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.6 | 0.8 | -0.1 | 0.5 | -0.2 | -23.0 |
| Assault | 5.6 | 5.1 | 0.5 | 5.2 | 0.6 | 13.5 |
| Robbery | 3.4 | 3.6 | -0.3 | 3.0 | -0.3 | -9.9 |
| Burglary | 4.3 | 4.9 | -0.7 | 3.5 | -0.9 | -19.9 |
| Larceny, vehicle theft, or other property crimes | 10.9 | 11.1 | -0.1 | 10.2 | -0.2 | -1.7 |
| Drug law violations | 10.8 | 12.2 | -1.4* | 10.2 | -1.9* | -15.4 |
| Other personal crimes | 7.0 | 6.8 | 0.1 | 7.0 | 0.2 | 2.7 |
| Other miscellaneous crimes** | 23.3 | 27.5 | -4.3*** | 21.4 | -5.7*** | -21.1 |
| Percentage Had a Serious Arrest Charge[e] | 11.6 | 12.6 | -1.0 | 10.5 | -1.3 | -10.8 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment** | 31.0 | 35.6 | -4.6*** | 28.9 | -6.1*** | -17.4 |
| Percentage Made a Deal or Plea-Bargained*** | 17.9 | 21.8 | -3.9*** | 15.9 | -5.2*** | -24.7 |

TABLE F.8 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 3.5 |
| Assault | 3.5 | 3.1 | 0.4 | 3.1 | 0.5 | 18.4 |
| Robbery | 2.2 | 2.8 | -0.6 | 1.6 | -0.8 | -34.3 |
| Burglary | 2.8 | 3.3 | -0.5 | 2.6 | -0.6 | -19.8 |
| Larceny, vehicle theft, or other property crimes | 7.9 | 8.0 | 0.0 | 7.6 | 0.0 | -0.6 |
| Drug law violations* | 8.3 | 9.5 | -1.1 | 7.6 | -1.5 | -16.6 |
| Other personal crimes | 4.3 | 4.4 | -0.1 | 4.5 | -0.1 | -2.4 |
| Other miscellaneous crimes | 16.6 | 18.9 | -2.3** | 15.2 | -3.0** | -16.6 |
| Percentage Ever Served Time in Jail for Convictions** | 23.0 | 26.6 | -3.6*** | 20.9 | -4.8*** | -18.5 |
| Average Weeks in Jail for Convictions* | 9.7 | 11.0 | -1.3* | 7.9 | -1.7* | -17.6 |
| Percentage Ever Put on Probation or Parole | 19.1 | 20.6 | -1.4 | 17.8 | -1.9 | -9.7 |
| **Sample Size** | **3,373** | **2,581** | **5,954** | **2,542** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two groups of residential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE F.9

## IMPACTS ON KEY CRIME OUTCOMES FOR FEMALE RESIDENTIAL DESIGNEES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1 | 0.9 | 1.8 | -0.9** | 0.7 | -1.2** | -65.3 |
| 2** | 1.5 | 1.3 | 0.2 | 1.2 | 0.2 | 24.8 |
| 3 | 1.6 | 2.3 | -0.6 | 1.6 | -0.9 | -35.9 |
| 4** | 1.7 | 1.7 | 0.1 | 1.0 | 0.1 | 8.2 |
| 5 | 2.0 | 2.0 | 0.0 | 1.8 | 0.1 | 3.9 |
| 6 | 0.8 | 2.0 | -1.1*** | 0.8 | -1.6*** | -67.2 |
| 7 | 1.0 | 1.6 | -0.7* | 1.0 | -0.9* | -49.3 |
| 8 | 1.2 | 1.4 | -0.2 | 1.2 | -0.3 | -20.9 |
| 9** | 1.1 | 2.4 | -1.3*** | 1.0 | -1.8*** | -63.9 |
| 10 | 1.7 | 1.9 | -0.2 | 1.5 | -0.3 | -14.7 |
| 11 | 1.4 | 1.2 | 0.2 | 1.2 | 0.3 | 34.7 |
| 12 | 0.9 | 1.1 | -0.2 | 0.9 | -0.3 | -25.9 |
| 13 | 1.2 | 1.0 | 0.3 | 1.1 | 0.4 | 51.5 |
| 14** | 1.1 | 0.6 | 0.5 | 0.8 | 0.7 | 476.7 |
| 15 | 1.1 | 1.2 | -0.1 | 0.9 | -0.2 | -15.9 |
| 16 | 1.9 | 1.9 | 0.0 | 1.5 | -0.1 | -4.2 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years*** | 16.7 | 17.5 | -0.8 | 15.1 | -1.1 | -6.9 |
| 1*** | 5.3 | 6.1 | -0.9 | 4.4 | -1.2 | -21.8 |
| 2 | 4.7 | 5.9 | -1.2 | 4.5 | -1.7 | -27.5 |
| 3 | 4.8 | 5.6 | -0.9 | 4.5 | -1.2 | -21.7 |
| 4** | 4.7 | 3.9 | 0.8 | 4.1 | 1.2 | 39.3 |
| Average Number of Times Ever Arrested | 0.3 | 0.3 | -0.1*** | 0.2 | -0.1*** | -30.7 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.1 | 0.1 | 0.1 | 0.1 | 0.1 | 559.7 |
| Assault | 2.1 | 2.6 | -0.5 | 2.4 | -0.7 | -23.3 |
| Robbery | 0.4 | 0.3 | 0.1 | 0.3 | 0.1 | 115.3 |
| Burglary | 0.5 | 0.6 | 0.0 | 0.3 | 0.0 | -14.0 |
| Larceny, vehicle theft, or other property crimes | 4.4 | 6.0 | -1.6** | 3.9 | -2.3** | -37.3 |
| Drug law violations | 2.3 | 2.1 | 0.2 | 1.8 | 0.3 | 19.5 |
| Other personal crimes | 3.4 | 3.5 | -0.2 | 3.1 | -0.3 | -7.6 |
| Other miscellaneous crimes** | 7.9 | 9.0 | -1.1 | 7.2 | -1.5 | -17.7 |
| Percentage Had a Serious Arrest Charge[e] | 3.1 | 3.3 | -0.3 | 2.8 | -0.4 | -12.1 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment** | 10.7 | 11.8 | -1.0 | 9.2 | -1.5 | -13.9 |
| Percentage Made a Deal or Plea-Bargained*** | 4.7 | 4.6 | 0.1 | 4.1 | 0.1 | 3.4 |

TABLE F.9 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | 220.2 |
| Assault | 1.0 | 1.4 | -0.4 | 0.9 | -0.6 | -39.3 |
| Robbery | 0.2 | 0.5 | -0.3 | 0.1 | -0.4 | -84.1 |
| Burglary | 0.3 | 0.4 | -0.1 | 0.1 | -0.2 | -59.5 |
| Larceny, vehicle theft, or other property crimes | 3.5 | 3.8 | -0.3 | 3.1 | -0.5 | -13.6 |
| Drug law violations* | 1.8 | 1.4 | 0.4 | 1.1 | 0.6 | 137.4 |
| Other personal crimes | 1.7 | 2.3 | -0.6 | 1.6 | -0.8 | -34.0 |
| Other miscellaneous crimes | 4.2 | 5.2 | -1.0 | 3.4 | -1.5 | -30.0 |
| Percentage Ever Served Time in Jail for Convictions** | 6.5 | 6.9 | -0.4 | 5.4 | -0.6 | -10.2 |
| Average Weeks in Jail for Convictions* | 1.0 | 0.8 | 0.2 | 0.6 | 0.2 | 57.9 |
| Percentage Ever Put on Probation or Parole | 6.2 | 6.6 | -0.4 | 4.9 | -0.6 | -11.1 |
| **Sample Size** | **2,097** | **1,163** | **3,260** | **1,506** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two groups of residential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE F.10

## IMPACTS ON KEY CRIME OUTCOMES FOR MALE NONRESIDENTIAL DESIGNEES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1* | 4.0 | 1.9 | 2.0 | 3.0 | 2.9 | 1,975.9 |
| 2 | 3.7 | 3.4 | 0.4 | 3.8 | 0.5 | 15.7 |
| 3 | 2.8 | 3.7 | -0.9 | 3.0 | -1.3 | -30.5 |
| 4 | 4.4 | 3.9 | 0.6 | 3.3 | 0.8 | 33.2 |
| 5 | 3.5 | 3.3 | 0.1 | 2.7 | 0.2 | 8.5 |
| 6 | 3.8 | 3.0 | 0.8 | 4.2 | 1.1 | 34.2 |
| 7 | 4.3 | 4.3 | 0.0 | 3.4 | 0.0 | -0.3 |
| 8 | 2.9 | 4.4 | -1.5 | 2.3 | -2.2 | -49.1 |
| 9 | 4.1 | 2.3 | 1.7 | 4.7 | 2.5 | 114.9 |
| 10 | 4.9 | 4.3 | 0.6 | 5.9 | 0.8 | 16.7 |
| 11 | 5.1 | 4.4 | 0.7 | 3.5 | 1.0 | 42.6 |
| 12 | 3.0 | 3.8 | -0.9 | 3.5 | -1.3 | -26.7 |
| 13 | 1.4 | 2.1 | -0.7 | 1.2 | -1.1 | -46.5 |
| 14 | 4.6 | 2.0 | 2.6* | 4.3 | 3.8* | 744.0 |
| 15 | 3.1 | 1.8 | 1.3 | 2.7 | 1.9 | 246.2 |
| 16 | 4.6 | 4.4 | 0.2 | 4.3 | 0.4 | 8.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years* | 37.2 | 33.1 | 4.1 | 35.9 | 5.8 | 19.4 |
| 1 | 13.9 | 12.5 | 1.4 | 12.7 | 2.0 | 18.9 |
| 2 | 13.6 | 13.7 | -0.1 | 11.5 | -0.1 | -0.7 |
| 3 | 14.3 | 12.5 | 1.9 | 14.8 | 2.7 | 22.1 |
| 4 | 11.8 | 9.1 | 2.7 | 10.4 | 3.9 | 59.5 |
| Average Number of Times Ever Arrested* | 0.8 | 0.7 | 0.1 | 0.8 | 0.2 | 31.3 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.8 | 0.5 | 0.3 | 1.1 | 0.5 | 71.0 |
| Assault | 5.6 | 2.9 | 2.7 | 6.1 | 3.9 | 176.0 |
| Robbery | 2.9 | 2.9 | 0.0 | 1.9 | 0.0 | 1.1 |
| Burglary | 3.8 | 2.9 | 0.9 | 4.3 | 1.3 | 43.5 |
| Larceny, vehicle theft, or other property crimes** | 9.8 | 5.8 | 4.0* | 8.4 | 5.8* | 223.4 |
| Drug law violations | 8.3 | 9.2 | -0.9 | 7.7 | -1.3 | -14.7 |
| Other personal crimes | 4.9 | 7.0 | -2.1 | 5.5 | -3.1 | -36.0 |
| Other miscellaneous crimes** | 22.8 | 18.3 | 4.5 | 20.8 | 6.4 | 44.7 |
| Percentage Had a Serious Arrest Charge[e] | 11.0 | 7.2 | 3.7 | 10.8 | 5.4 | 99.4 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment* | 29.9 | 26.2 | 3.6 | 28.1 | 5.2 | 22.8 |
| Percentage Made a Deal or Plea-Bargained | 17.5 | 14.6 | 2.8 | 14.2 | 4.1 | 40.6 |

TABLE F.10 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.8 | 0.5 | 0.4 | 0.8 | 0.5 | 190.1 |
| Assault | 2.5 | 1.5 | 1.0 | 2.4 | 1.4 | 148.2 |
| Robbery | 2.2 | 2.0 | 0.2 | 0.7 | 0.3 | 73.6 |
| Burglary | 2.0 | 2.9 | -1.0 | 2.0 | -1.4 | -41.1 |
| Larceny, vehicle theft, or other property crimes | 6.4 | 4.4 | 1.9 | 5.2 | 2.8 | 117.7 |
| Drug law violations | 7.0 | 6.9 | 0.0 | 6.0 | 0.1 | 0.9 |
| Other personal crimes | 4.5 | 4.7 | -0.2 | 4.8 | -0.3 | -6.5 |
| Other miscellaneous crimes | 16.5 | 14.0 | 2.5 | 15.0 | 3.6 | 31.9 |
| Percentage Ever Served Time in Jail for Convictions | 21.7 | 17.8 | 3.9 | 19.9 | 5.5 | 38.7 |
| Average Weeks in Jail for Convictions | 7.5 | 5.8 | 1.7 | 6.5 | 2.5 | 62.7 |
| Percentage Ever Put on Probation or Parole | 17.7 | 17.6 | 0.2 | 16.8 | 0.2 | 1.3 |
| **Sample Size** | **368** | **206** | **574** | **257** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two groups of nonresidential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

### TABLE F.11

### IMPACTS ON KEY CRIME OUTCOMES FOR FEMALE NONRESIDENTIAL DESIGNEES

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Quarter After Random Assignment | | | | | | |
| 1* | 0.4 | 1.0 | -0.6 | 0.0 | -1.0 | -100.8 |
| 2 | 0.6 | 1.8 | -1.2** | 0.7 | -1.9** | -74.4 |
| 3 | 0.9 | 1.6 | -0.6 | 0.7 | -1.1 | -61.6 |
| 4 | 1.0 | 1.8 | -0.8 | 1.0 | -1.3 | -56.0 |
| 5 | 0.8 | 1.5 | -0.7 | 0.3 | -1.1 | -77.8 |
| 6 | 0.8 | 0.5 | 0.3 | 1.0 | 0.4 | 72.5 |
| 7 | 0.6 | 0.6 | 0.0 | 0.5 | 0.1 | 14.4 |
| 8 | 0.9 | 0.5 | 0.3 | 0.7 | 0.6 | 590.0 |
| 9 | 0.9 | 0.8 | 0.1 | 1.3 | 0.2 | 14.9 |
| 10 | 0.8 | 1.2 | -0.4 | 1.0 | -0.6 | -38.6 |
| 11 | 1.3 | 1.3 | 0.0 | 1.2 | 0.0 | 4.5 |
| 12 | 0.8 | 0.9 | -0.1 | 1.0 | -0.1 | -13.1 |
| 13 | 0.9 | 1.4 | -0.5 | 0.7 | -0.9 | -56.8 |
| 14 | 0.9 | 0.9 | 0.0 | 0.7 | 0.0 | 0.2 |
| 15 | 0.7 | 1.8 | -1.0* | 0.5 | -1.7* | -77.6 |
| 16 | 1.1 | 1.4 | -0.3 | 1.3 | -0.5 | -29.6 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint, by Year | | | | | | |
| All years* | 9.5 | 13.3 | -3.8** | 8.7 | -6.1** | -41.3 |
| 1 | 2.7 | 5.5 | -2.8*** | 2.2 | -4.5*** | -67.6 |
| 2 | 2.9 | 3.2 | -0.3 | 2.5 | -0.4 | -14.8 |
| 3 | 3.5 | 3.7 | -0.2 | 4.1 | -0.3 | -6.9 |
| 4 | 3.0 | 4.6 | -1.6 | 2.7 | -2.6 | -48.7 |
| Average Number of Times Ever Arrested* | 0.2 | 0.2 | -0.1** | 0.1 | -0.1** | -50.8 |
| All Charges for Which Arrested (Percentages) | | | | | | |
| Murder | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | n.a. |
| Assault | 1.5 | 0.9 | 0.6 | 1.5 | 1.0 | 183.9 |
| Robbery | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | n.a. |
| Burglary | 0.1 | 0.2 | -0.1 | 0.0 | -0.1 | -102.3 |
| Larceny, vehicle theft, or other property crimes** | 2.3 | 4.3 | -2.0** | 2.1 | -3.2** | -60.0 |
| Drug law violations | 1.5 | 1.0 | 0.5 | 1.2 | 0.9 | 248.7 |
| Other personal crimes | 1.7 | 2.9 | -1.2 | 1.8 | -2.0 | -51.6 |
| Other miscellaneous crime** | 4.9 | 7.6 | -2.7** | 4.6 | -4.4** | -49.0 |
| Percentage Had a Serious Arrest Charge[e] | 1.6 | 1.0 | 0.5 | 1.5 | 0.9 | 140.6 |
| Percentage Convicted, Pled Guilty, or Adjudged Delinquent During the 48 Months After Random Assignment* | 5.3 | 8.5 | -3.2** | 4.5 | -5.3** | -54.0 |
| Percentage Made a Deal or Plea-Bargained | 2.8 | 2.8 | 0.0 | 2.6 | 0.0 | -0.4 |

TABLE F.11 *(continued)*

| Outcome Measure[a] | Program Group | Control Group | Estimated Impact per Eligible Applicant[b] | Program Group Job Corps Participants | Estimated Impact per Participant[c] | Percentage Gain from Participation[d] |
|---|---|---|---|---|---|---|
| All Charges for Which Convicted (Percentages) | | | | | | |
| Murder | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | n.a. |
| Assault | 0.5 | 0.3 | 0.2 | 0.2 | 0.3 | -518.6 |
| Robbery | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | n.a. |
| Burglary | 0.1 | 0.0 | 0.1 | 0.0 | 0.2 | -98.9 |
| Larceny, vehicle theft, or other property crimes | 1.5 | 3.0 | -1.5** | 1.5 | -2.4** | -61.8 |
| Drug law violations | 1.1 | 0.7 | 0.4 | 0.9 | 0.7 | 328.0 |
| Other personal crimes | 0.8 | 0.9 | -0.1 | 0.7 | -0.1 | -15.3 |
| Other miscellaneous crimes | 2.0 | 4.1 | -2.1** | 1.7 | -3.4** | -66.8 |
| Percentage Ever Served Time in Jail for Convictions | 3.0 | 4.2 | -1.2 | 2.5 | -2.0 | -43.7 |
| Average Weeks in Jail for Convictions | 0.6 | 0.3 | 0.2 | 0.4 | 0.4 | 1,768.5 |
| Percentage Ever Put on Probation or Parole | 3.3 | 5.5 | -2.2** | 2.7 | -3.6** | -56.7 |
| **Sample Size** | **968** | **521** | **1,489** | **608** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

n.a. = not applicable.

[a] Asterisks next to variable names indicate significance levels for statistical tests of differences in impacts across the two groups of nonresidential designees.

[b] Estimated impacts per eligible applicant are measured as the difference between the weighted mean for program and control group members.

[c] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[d] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[e] The serious arrest charges are murder, assault, robbery, and burglary.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE F.12

## IMPACTS ON KEY CRIME OUTCOMES, BY THE PRESENCE OF CHILDREN, HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE

| | Percentage Ever Arrested | | Percentage Arrested for Serious Crimes (Assault, Murder, Robbery, or Burglary) | | Percentage Ever Convicted, Pled Guilty, or Adjudged Delinquent | | Percentage Ever Incarcerated for Convictions | | Average Weeks Incarcerated for Convictions | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] |
| Presence of Children at Random Assignment for Females | | | | | | | | | | |
| Had children | 15.7 | -4.9* | 2.2 | -0.2 | 10.4 | -3.7 | 5.9 | -1.6 | 0.6 | 0.9 |
| Had no children | 16.8 | -1.2 | 3.0 | -0.2 | 11.3 | -2.0 | 6.5 | -0.9 | 0.8 | 0.0 |
| (P-value)[b] | | .344 | | .953 | | .653 | | .818 | | .271 |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 20.2 | -6.0*** | 3.1 | 0.4 | 14.7 | -3.5* | 9.2 | -0.8 | 2.5 | 0.2 |
| Had no high school credential | 36.5 | -5.1*** | 10.0 | -1.0 | 28.5 | -4.6*** | 20.7 | -3.7*** | 7.9 | -1.2 |
| (P-value)[b] | | .888 | | .286 | | .530 | | .128 | | .220 |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 26.2 | -5.3*** | 6.4 | -1.1 | 19.3 | -4.4*** | 12.8 | -2.6*** | 4.3 | -0.9 |
| Ever arrested for nonserious crimes only | 45.7 | -1.3 | 11.0 | -0.3 | 36.1 | -0.4 | 26.7 | -0.6 | 10.3 | -0.3 |
| Ever arrested for serious crimes[c] | 56.1 | -4.7 | 19.8 | 2.9 | 44.8 | -0.2 | 36.8 | -0.9 | 14.3 | -0.1 |
| (P-value)[b] | | .470 | | .706 | | .377 | | .760 | | .932 |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 36.5 | -5.9** | 8.0 | -0.2 | 30.3 | -4.5* | 20.0 | -2.6 | 5.5 | -0.4 |
| Black non-Hispanic | 32.7 | -5.4*** | 9.4 | -1.0 | 24.4 | -4.8*** | 18.4 | -4.2*** | 7.9 | -1.4 |
| Hispanic | 26.3 | -2.5 | 6.8 | 0.3 | 19.8 | -1.7 | 14.2 | 0.7 | 5.5 | 0.1 |
| Other[d] | 32.4 | -9.0** | 7.1 | -1.7 | 24.7 | -7.4* | 16.6 | -5.4 | 4.3 | -0.7 |
| (P-value)[b] | | .604 | | .865 | | .596 | | .251 | | .818 |

TABLE F.12 *(continued)*

| | Percentage Ever Arrested | | Percentage Arrested for Serious Crimes (Assault, Murder, Robbery, or Burglary) | | Percentage Ever Convicted, Pled Guilty, or Adjudged Delinquent | | Percentage Ever Incarcerated for Convictions | | Average Weeks Incarcerated for Convictions | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 32.9 | -4.3* | 8.3 | 1.0 | 25.1 | -2.2 | 16.1 | 1.9 | 6.1 | 1.6 |
| On or after 3/1/95 (after ZT) | 32.5 | -5.5*** | 8.4 | -1.1 | 25.2 | -4.9*** | 18.5 | -4.4*** | 6.7 | -1.5** |
| (P-value)[b] | | .663 | | .246 | | .291 | | .008*** | | .039** |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[b] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[c] Serious crimes include aggravated assault, murder, robbery, and burglary.

[d] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# APPENDIX G

# SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON CRIMES COMMITTED AGAINST JOB CORPS PARTICIPANTS

## TABLE G.1

## IMPACTS ON THE NUMBER OF VICTIMIZATIONS IN THE PREVIOUS YEAR, BY CRIME TYPE

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| Average Number of Victimizations at 12 Months, by Crime Type | | | | | | |
| Assault | 0.220 | 0.228 | -0.008 | 0.206 | -0.011 | -5.2 |
| Burglary | 0.058 | 0.077 | -0.019** | 0.046 | -0.026** | -36.3 |
| Robbery | 0.084 | 0.103 | -0.019* | 0.088 | -0.026* | -22.9 |
| Larceny[d] | 0.147 | 0.186 | -0.039** | 0.155 | -0.054** | -25.8 |
| Motor vehicle theft | 0.019 | 0.024 | -0.005 | 0.018 | -0.007 | -27.3 |
| Average Number of Victimizations at 30 Months, by Crime Type | | | | | | |
| Assault | 0.182 | 0.188 | -0.006 | 0.187 | -0.008 | -4.2 |
| Burglary | 0.054 | 0.087 | -0.033*** | 0.049 | -0.046*** | -48.0 |
| Robbery | 0.056 | 0.091 | -0.035*** | 0.060 | -0.048*** | -44.8 |
| Larceny[d] | 0.133 | 0.118 | 0.015 | 0.144 | 0.020 | 16.3 |
| Motor vehicle theft | 0.029 | 0.027 | 0.002 | 0.027 | 0.003 | 12.7 |
| Average Number of Victimizations at 48 Months, by Crime Type | | | | | | |
| Assault | 0.158 | 0.161 | -0.003 | 0.161 | -0.004 | -2.5 |
| Burglary | 0.055 | 0.053 | 0.001 | 0.055 | 0.002 | 3.7 |
| Robbery | 0.056 | 0.066 | -0.011 | 0.061 | -0.015 | -19.8 |
| Larceny[d] | 0.104 | 0.115 | -0.010 | 0.093 | -0.015 | -13.6 |
| Motor vehicle theft | 0.026 | 0.034 | -0.008 | 0.024 | -0.011 | -32.4 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impact per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

[d] Larceny includes pickpocketing, purse snatching, extortion, and theft from or damage to motor vehicles.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE G.2

IMPACTS ON KEY VICTIMIZATION OUTCOMES, BY AGE, GENDER, RESIDENTIAL DESIGNATION STATUS, HIGH SCHOOL CREDENTIAL STATUS, ARREST HISTORY, RACE AND ETHNICITY, AND APPLICATION DATE

| | Percentage Victimized at 12 Months | | Percentage Victimized at 30 Months | | Average Number of Incidents at 12 Months | | Average Number of Incidents at 30 Months | | Average Number of Incidents at 48 Months | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] |
| Age at Application | | | | | | | | | | |
| 16 and 17 | 24.5 | -0.3 | 23.6 | -2.3 | 0.57 | -0.07 | 0.53 | 0.01 | 0.40 | 0.14 |
| 18 and 19 | 24.9 | -5.9*** | 22.0 | -5.2** | 0.48 | -0.14** | 0.48 | -0.11 | 0.52 | -0.27 |
| 20 to 24 | 22.7 | -4.7** | 19.6 | -2.1 | 0.60 | -0.36*** | 0.36 | -0.02 | 0.33 | 0.02 |
| (P-value)[b] | | .112 | | .507 | | .228 | | .820 | | .163 |
| Gender | | | | | | | | | | |
| Males | 25.6 | -2.1 | 24.2 | -4.8*** | 0.53 | 0.06 | 0.48 | -0.01 | 0.53 | -0.16 |
| Females | 22.2 | -4.8*** | 19.0 | -0.8 | 0.58 | -0.33*** | 0.45 | -0.07 | 0.26 | 0.21* |
| (P-value)[b] | | .300 | | .066* | | .019** | | .690 | | .022* |
| Residential Designees | | | | | | | | | | |
| Males | 25.5 | -2.0 | 24.0 | -4.6*** | 0.53 | -0.05 | 0.49 | -0.01 | 0.54 | -0.15 |
| Females | 22.5 | -5.2** | 18.6 | -1.8 | 0.59 | -0.36*** | 0.45 | -0.13 | 0.27 | 0.22 |
| (P-value)[b] | | .235 | | .241 | | .012** | | .492 | | .047** |
| Nonresidential Designees | | | | | | | | | | |
| Males | 25.7 | -4.1 | 26.8 | -7.0 | 0.51 | -0.16 | 0.39 | 0.01 | 0.43 | -0.28** |
| Females | 21.1 | -3.2 | 20.2 | 3.0 | 0.55 | -0.23 | 0.45 | 0.13 | 0.25 | 0.18 |
| (P-value)[b] | | .845 | | .131 | | .878 | | .607 | | .009*** |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 24.3 | -9.9*** | 19.2 | -4.2* | 0.57 | -0.37*** | 0.43 | -0.19** | 0.28 | 0.05 |
| Had no high school credential | 24.2 | -1.3 | 22.9 | -3.0** | 0.55 | -0.10* | 0.49 | 0.00 | 0.46 | -0.03 |
| (P-value)[b] | | .002*** | | .733 | | .093* | | .153 | | .542 |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 22.9 | -3.6*** | 20.4 | -2.8** | 0.52 | -0.19*** | 0.46 | -0.12 | 0.43 | -0.10 |
| Ever arrested for nonserious crimes only | 25.8 | 0.2 | 26.4 | -4.7 | 0.54 | 0.01 | 0.51 | 0.02 | 0.45 | 0.07 |
| Ever arrested for serious crimes | 31.3 | -5.5 | 27.1 | -4.2 | 0.78 | 0.09 | 0.47 | 0.39 | 0.44 | 0.47 |
| (P-value)[b] | | .441 | | .851 | | .234 | | .440 | | .440 |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 26.7 | -3.5 | 23.7 | -4.2* | 0.68 | -0.18 | 0.48 | 0.00 | 0.53 | -0.12 |
| Black non-Hispanic | 25.4 | -5.9*** | 22.2 | -3.4** | 0.54 | -0.19*** | 0.54 | -0.18 | 0.34 | 0.03 |
| Hispanic | 19.2 | 3.0 | 20.0 | -1.6 | 0.48 | -0.18* | 0.31 | 0.28 | 0.57 | -0.22 |
| Other | 19.2 | 0.8 | 20.1 | -2.6 | 0.33 | 0.10 | 0.38 | -0.01 | 0.20 | 0.53 |
| (P-value)[b] | | .023** | | .905 | | .176 | | .249 | | .369 |

TABLE G.2 *(continued)*

| Subgroup | Percentage Victimized at 12 Months | | Percentage Victimized at 30 Months | | Average Number of Incidents at 12 Months | | Average Number of Incidents at 30 Months | | Average Number of Incidents at 48 Months | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] | Control Group | Estimated Impact per Participant[a] |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 24.8 | -3.1 | 21.5 | -1.2 | 0.64 | -0.27* | 0.55 | -0.19 | 0.56 | -0.09 |
| On or after 3/1/95 (after ZT) | 24.0 | -3.2** | 22.2 | -3.8*** | 0.53 | -0.13** | 0.45 | 0.01 | 0.38 | 0.00 |
| (P-value)[b] | | .930 | | .321 | | .361 | | .280 | | .752 |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per program participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[b] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[c] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# APPENDIX H

# SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE

## TABLE H.1

## FREQUENCY OF TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 30-MONTH INTERVIEW

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| How Often Smoked Cigarettes | | | | | | |
| Not at all | 47.4 | 48.5 | -1.1 | 46.9 | -1.5 | -3.0 |
| Less than once a week | 3.0 | 2.9 | 0.1 | 3.2 | 0.1 | 3.6 |
| 1 to 2 days per week | 2.8 | 3.1 | -0.3 | 3.0 | -0.4 | -13.0 |
| 3 or more days per week | 46.8 | 45.5 | 1.3 | 46.9 | 1.8 | 4.0 |
| How Often Consumed Alcoholic Beverages | | | | | | |
| Not at all | 66.8 | 66.8 | 0.0 | 66.5 | -0.1 | -0.1 |
| Less than once a week | 17.5 | 17.2 | 0.3 | 17.0 | 0.4 | 2.3 |
| 1 to 2 days per week | 10.9 | 11.3 | -0.4 | 11.3 | -0.5 | -4.4 |
| 3 or more days per week | 4.8 | 4.7 | 0.1 | 5.1 | 0.2 | 4.1 |
| How Often Used Marijuana or Hashish | | | | | | |
| Not at all | 91.8 | 91.6 | 0.2 | 91.2 | 0.3 | 0.3 |
| Less than once a week | 2.1 | 2.4 | -0.3 | 2.1 | -0.4 | -17.5 |
| 1 to 2 days per week | 2.0 | 1.6 | 0.4 | 2.1 | 0.5 | 31.3 |
| 3 or more days per week | 4.2 | 4.5 | -0.3 | 4.5 | -0.4 | -7.5 |
| How Often Snorted Cocaine Powder | | | | | | |
| Not at all | 99.7 | 99.6 | 0.0 | 99.6 | 0.0 | 0.0 |
| Less than once a week | 0.2 | 0.3 | -0.1 | 0.2 | -0.2 | -54.3 |
| 1 to 2 days per week | 0.1 | 0.0 | 0.0 | 0.0 | 0.0 | 452.2 |
| 3 or more days per week | 0.1 | 0.0 | 0.1 | 0.1 | 0.1 | 303.3 |
| How Often Smoked Crack Cocaine or Freebased | | | | | | |
| Not at all | 99.9 | 99.9 | 0.0 | 99.9 | 0.0 | 0.0 |
| Less than once a week | 0.1 | 0.1 | 0.0 | 0.0 | 0.0 | -65.3 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -23.2 |
| 3 or more days per week | 0.1 | 0.0 | 0.1 | 0.0 | 0.1 | -272.5 |
| How Often Used Hallucinogenic Drugs | | | | | | |
| Not at all | 99.4 | 99.4 | 0.0 | 99.3 | 0.0 | 0.0 |
| Less than once a week | 0.4 | 0.5 | -0.1 | 0.4 | -0.1 | -20.8 |
| 1 to 2 days per week | 0.2 | 0.1 | 0.0 | 0.2 | 0.0 | 25.0 |
| 3 or more days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.1 | -146.6 |
| How Often Used Heroin, Opium, Methadone, or Downers | | | | | | |
| Not at all | 99.8 | 99.8 | 0.1 | 99.9 | 0.1 | 0.1 |
| Less than once a week | 0.1 | 0.1 | 0.0 | 0.0 | -0.1 | -54.6 |
| 1 to 2 days per week | 0.1 | 0.1 | 0.0 | 0.0 | 0.0 | -52.4 |
| 3 or more days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -29.1 |
| How Often Used Speed, Uppers, or Methamphetamines | | | | | | |
| Not at all | 99.5 | 99.4 | 0.1 | 99.4 | 0.1 | 0.1 |
| Less than once a week | 0.3 | 0.5 | -0.2 | 0.4 | -0.3 | -40.1 |
| 1 to 2 days per week | 0.1 | 0.1 | 0.0 | 0.1 | 0.1 | 242.1 |
| 3 or more days per week | 0.1 | 0.0 | 0.1 | 0.1 | 0.1 | 161.6 |

TABLE H.1 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| How Often Used Other Drugs | | | | | | |
| Not at all | 99.9 | 99.9 | 0.0 | 99.8 | 0.0 | 0.0 |
| Less than once a week | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | 3.3 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 3,414.7 |
| 3 or more days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -8.8 |
| How Often Shot or Injected Drugs with a Needle or Syringe | | | | | | |
| Not at all | 99.9 | 99.9 | 0.0 | 100 | 0.0 | 0.0 |
| Less than once a week | 0.0 | 0.1 | 0.0 | 0.0 | 0.0 | -60.4 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -95.7 |
| 3 or more days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -18.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 30-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE H.2

## FREQUENCY OF TOBACCO, ALCOHOL, AND ILLEGAL DRUG USE IN THE 30 DAYS PRIOR TO THE 48-MONTH INTERVIEW

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| How Often Smoked Cigarettes | | | | | | |
| Not at all | 49.9 | 48.6 | 1.3 | 50.0 | 1.8 | 3.6 |
| Less than once a week | 2.9 | 3.4 | -0.5 | 2.9 | -0.7 | -19.7 |
| 1 to 2 days per week | 2.4 | 2.6 | -0.2 | 2.5 | -0.3 | -9.5 |
| 3 or more days per week | 44.8 | 45.4 | -0.6 | 44.5 | -0.8 | -1.7 |
| How Often Consumed Alcoholic Beverages | | | | | | |
| Not at all | 64.1 | 64.6 | -0.5 | 63.6 | -0.7 | -1.1 |
| Less than once a week | 17.9 | 18.4 | -0.5 | 17.9 | -0.7 | -3.6 |
| 1 to 2 days per week | 11.9 | 10.9 | 1.0 | 12.2 | 1.3 | 12.3 |
| 3 or more days per week | 6.0 | 6.0 | 0.0 | 6.3 | 0.1 | 0.9 |
| How Often Used Marijuana or Hashish | | | | | | |
| Not at all | 92.9 | 92.7 | 0.2*** | 92.8 | 0.3*** | 0.3 |
| Less than once a week | 2.0 | 1.9 | 0.1 | 2.1 | 0.1 | 6.4 |
| 1 to 2 days per week | 0.9 | 1.6 | -0.7 | 0.8 | -1.0 | -56.2 |
| 3 or more days per week | 4.2 | 3.7 | 0.4 | 4.3 | 0.6 | 15.6 |
| How Often Snorted Cocaine Powder | | | | | | |
| Not at all | 99.7 | 99.8 | -0.2 | 99.7 | -0.2 | -0.2 |
| Less than once a week | 0.2 | 0.1 | 0.1 | 0.2 | 0.1 | 149.1 |
| 1 to 2 days per week | 0.1 | 0.0 | 0.0 | 0.0 | 0.0 | 4,930.1 |
| 3 or more days per week | 0.1 | 0.0 | 0.1 | 0.1 | 0.1 | -490.1 |
| How Often Smoked Crack Cocaine or Freebased | | | | | | |
| Not at all | 99.9 | 99.9 | 0.0 | 99.9 | 0.0 | 0.0 |
| Less than once a week | 0.0 | 0.1 | 0.0 | 0.1 | 0.0 | -18.1 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -105.3 |
| 3 or more days per week | 0.0 | 0.1 | 0.0 | 0.0 | 0.0 | -56.0 |
| How Often Used Hallucinogenic Drugs | | | | | | |
| Not at all | 99.7 | 99.3 | 0.4** | 99.7 | 0.5** | 0.5 |
| Less than once a week | 0.3 | 0.4 | -0.2 | 0.3 | -0.3 | -48.3 |
| 1 to 2 days per week | 0.0 | 0.1 | -0.1 | 0.0 | -0.1 | -84.5 |
| 3 or more days per week | 0.0 | 0.1 | -0.1 | 0.0 | -0.1 | -73.2 |
| How Often Used Heroin, Opium, Methadone, or Downers | | | | | | |
| Not at all | 99.9 | 99.8 | 0.0 | 99.9 | 0.0 | 0.0 |
| Less than once a week | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | 85.5 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -46.8 |
| 3 or more days per week | 0.0 | 0.1 | -0.1 | 0.0 | -0.1 | -79.7 |
| How Often Used Speed, Uppers, or Methamphetamines | | | | | | |
| Not at all | 99.7 | 99.5 | 0.1 | 99.8 | 0.2 | 0.2 |
| Less than once a week | 0.2 | 0.3 | -0.1 | 0.1 | -0.1 | -45.7 |
| 1 to 2 days per week | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | -10.6 |
| 3 or more days per week | 0.1 | 0.1 | -0.1 | 0.0 | -0.1 | -67.2 |

TABLE H.2 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact per Eligible Applicant[a] | Program Group Job Corps Participants | Estimated Impact per Participant[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| How Often Used Other Drugs | | | | | | |
| Not at all | 99.9 | 99.8 | 0.0 | 99.8 | 0.0 | 0.0 |
| Less than once a week | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | -23.3 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 |
| 3 or more days per week | 0.0 | 0.1 | 0.0 | 0.0 | -0.1 | -59.1 |
| How Often Shot or Injected Drugs with a Needle or Syringe | | | | | | |
| Not at all | 100.0 | 99.8 | 0.1 | 100.0 | 0.2 | 0.2 |
| Less than once a week | 0.0 | 0.1 | -0.1 | 0.0 | -0.1 | 0.0 |
| 1 to 2 days per week | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -116.1 |
| 3 or more days per week | 0.0 | 0.1 | 0.0 | 0.0 | 0.0 | -101.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per eligible applicant are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts per Job Corps participant are measured as the estimated impacts per eligible applicant divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact per participant divided by the difference between the mean outcome for participants and the estimated impact per participant.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE H.3

## IMPACTS ON KEY ALCOHOL AND ILLEGAL DRUG USE OUTCOMES IN THE 30 DAYS PRIOR TO THE 12-MONTH INTERVIEW AND ON HEALTH STATUS AT 12 MONTHS, BY SUBGROUP

| | Percentage Consumed Alcoholic Beverages | | Percentage Used Marijuana or Hashish | | Percentage Used Hard Drugs[a] | | Percentage Used Marijuana/Hashish or Hard Drugs[a] | | Percentage with Fair or Poor Health | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Age at Application | | | | | | | | | | |
| 16 and 17 | 25.3 | -1.4 | 12.2 | 0.4 | 1.9 | 0.8 | 12.6 | 0.7 | 16.1 | -1.6 |
| 18 and 19 | 28.3 | -2.3 | 8.3 | 0.7 | 1.9 | -0.1 | 9.0 | 0.0 | 17.7 | -2.9 |
| 20 to 24 | 37.5 | -4.1 | 4.4 | 2.0 | 1.1 | -0.4 | 5.0 | 1.4 | 19.1 | -7.7*** |
| (P-value)[c] | | .761 | | .700 | | .332 | | .771 | | .073* |
| Gender | | | | | | | | | | |
| Males | 34.1 | -1.8 | 10.9 | 1.1 | 2.2 | 0.4 | 11.7 | 0.7 | 16.1 | -4.5*** |
| Females | 22.9 | -2.9 | 6.1 | 0.3 | 1.0 | -0.1 | 6.3 | 0.5 | 19.3 | -2.0 |
| (P-value)[c] | | .737 | | .569 | | .473 | | .905 | | .182 |
| Residential Designees | | | | | | | | | | |
| Males | 34.6 | -2.7 | 11.3 | 0.9 | 2.3 | 0.5 | 12.1 | 0.5 | 16.3 | -4.7*** |
| Females | 23.3 | -1.9 | 7.0 | -0.4 | 1.1 | 0.1 | 7.1 | 0.1 | 19.7 | -2.6 |
| (P-value)[c] | | .731 | | .460 | | .576 | | .805 | | .316 |
| Nonresidential Designees | | | | | | | | | | |
| Males | 26.7 | 10.6* | 5.6 | 4.2 | 1.0 | -1.0 | 6.6 | 2.8 | 14.2 | -1.2 |
| Females | 21.5 | -6.3* | 3.2 | 2.7 | 0.7 | -0.6 | 3.7 | 2.1 | 17.9 | 0.1 |
| (P-value)[c] | | .013** | | .623 | | .660 | | .806 | | .797 |
| Presence of Children at Random Assignment for Females | | | | | | | | | | |
| Had children | 29.8 | -4.9 | 5.3 | 2.9* | 1.4 | -0.6 | 6.1 | 2.0 | 18.7 | -3.4 |
| Had no children | 29.4 | -1.8 | 9.8 | 0.3 | 1.8 | 0.4 | 10.3 | 0.3 | 17.2 | -3.6*** |
| (P-value)[c] | | .416 | | .213 | | .239 | | .414 | | .784 |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 38.5 | 5.3 | 7.0 | 2.1 | 5.1 | -7.6*** | 8.8 | -0.5 | 18.9 | -5.7 |
| Had no high school credential | 27.6 | -1.1 | 9.8 | 1.4 | 1.7 | 0.7 | 10.3 | 1.4 | 17.7 | -3.1*** |
| (P-value)[c] | | .329 | | .866 | | .000*** | | .600 | | .661 |

TABLE H.3 *(continued)*

| Subgroup | Percentage Consumed Alcoholic Beverages | | Percentage Used Marijuana or Hashish | | Percentage Used Hard Drugs[a] | | Percentage Used Marijuana/Hashish or Hard Drugs[a] | | Percentage with Fair or Poor Health | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 26.8 | -1.8 | 7.1 | 0.9 | 1.3 | -0.3 | 7.6 | 0.7 | 17.0 | -3.0*** |
| Ever arrested for nonserious crimes only | 37.5 | -1.3 | 15.2 | -0.4 | 1.7 | 3.2*** | 15.9 | -0.7 | 18.8 | -6.1** |
| Ever arrested for serious crimes[c] | 33.8 | -0.6 | 11.5 | 5.3 | 4.6 | -1.3 | 14.1 | 2.1 | 15.5 | 3.5 |
| (P-value)[c] | | .972 | | .543 | | .014** | | .821 | | .207 |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 35.3 | -0.2 | 9.7 | 0.9 | 3.8 | -0.1 | 10.8 | 0.4 | 19.6 | -6.7*** |
| Black non-Hispanic | 25.5 | -1.1 | 8.7 | 1.2 | 0.5 | -0.1 | 9.0 | 1.0 | 16.9 | -3.4** |
| Hispanic | 31.8 | -6.2** | 8.3 | -1.7 | 1.7 | 1.0 | 8.8 | -1.7 | 14.7 | 1.3 |
| Other[d] | 28.8 | -7.9* | 8.5 | 3.3 | 2.1 | 1.2 | 9.6 | 3.7 | 19.3 | -4.4 |
| (P-value)[c] | | .200 | | .410 | | .567 | | .408 | | .082* |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 29.0 | -0.1 | 8.3 | 3.6** | 1.8 | 0.7 | 9.2 | 3.0* | 17.5 | -2.6 |
| On or after 3/1/95 (after ZT) | 29.6 | -2.9** | 9.1 | 0.0 | 1.7 | 0.1 | 9.6 | -0.1 | 17.4 | -3.8*** |
| (P-value)[c] | | .337 | | .060* | | .487 | | .124 | | .580 |

SOURCE: 12-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Hard drugs include cocaine powder, crack, speed/uppers/methamphetamines, hallucinogens, and heroin/opium/methadone/downers.

[b] Estimated impacts per program participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of eligible applicants in the program group who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[d] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE H.4

## IMPACTS ON KEY ALCOHOL AND ILLEGAL DRUG USE OUTCOMES IN THE 30 DAYS PRIOR TO THE 30-MONTH INTERVIEW AND ON HEALTH STATUS AT 30 MONTHS, BY SUBGROUP

| | Percentage Consumed Alcoholic Beverages | | Percentage Used Marijuana or Hashish | | Percentage Used Hard Drugs[a] | | Percentage Used Marijuana/Hashish or Hard Drugs[a] | | Percentage with Fair or Poor Health | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Age at Application | | | | | | | | | | |
| 16 and 17 | 29.4 | 1.6 | 11.5 | 0.4 | 1.9 | 0.7 | 11.9 | 0.5 | 16.9 | -1.3 |
| 18 and 19 | 35.0 | -0.2 | 6.9 | 0.2 | 2.0 | -0.2 | 7.3 | 0.5 | 16.4 | -1.4 |
| 20 to 24 | 37.0 | -2.4 | 5.6 | -1.9 | 1.1 | -0.3 | 5.8 | -1.6 | 17.9 | -4.8** |
| (P-value)[c] | | .475 | | .379 | | .440 | | .434 | | .415 |
| Gender | | | | | | | | | | |
| Males | 38.6 | 1.5 | 10.8 | 0.0 | 2.2 | 0.3 | 11.0 | 0.5 | 15.3 | -2.6** |
| Females | 25.6 | -1.9 | 5.2 | -0.7 | 1.0 | -0.2 | 5.8 | -0.9 | 19.3 | -1.6 |
| (P-value)[c] | | .201 | | .626 | | .450 | | .383 | | .593 |
| Residential Designees | | | | | | | | | | |
| Males | 39.0 | 0.9 | 11.3 | -0.5 | 2.3 | 0.4 | 11.6 | -0.1 | 15.6 | -3.3*** |
| Females | 26.4 | -2.0 | 6.0 | -1.3 | 1.1 | 0.0 | 6.6 | -1.5 | 19.7 | -2.1 |
| (P-value)[c] | | .323 | | .637 | | .547 | | .439 | | .564 |
| Nonresidential Designees | | | | | | | | | | |
| Males | 32.9 | 10.0 | 4.0 | 6.7** | 1.1 | -1.0 | 4.0 | 7.2** | 12.1 | 7.7* |
| Females | 23.2 | -1.1 | 2.6 | 1.5 | 0.7 | -0.7 | 3.0 | 1.2 | 18.0 | 0.1 |
| (P-value)[c] | | .116 | | .126 | | .696 | | .084* | | .165 |
| Presence of Children at Random Assignment for Females | | | | | | | | | | |
| Had children | 30.6 | -2.2 | 4.7 | -0.1 | 1.5 | -0.7 | 5.0 | 0.2 | 21.4 | -5.3* |
| Had no children | 33.9 | 0.5 | 9.2 | -0.4 | 1.8 | 0.3 | 9.6 | -0.3 | 15.9 | -1.4 |
| (P-value)[c] | | .452 | | .831 | | .268 | | .784 | | .228 |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 41.5 | 2.4 | 8.7 | -1.9 | 5.5 | -8.2*** | 9.9 | -3.4 | 17.2 | 2.2 |
| Had no high school credential | 32.1 | 0.8 | 9.3 | 0.1 | 1.7 | 0.6 | 9.6 | 0.4 | 17.7 | -2.4** |
| (P-value)[c] | | .829 | | .617 | | .000*** | | .342 | | .377 |

TABLE H.4 *(continued)*

| Subgroup | Percentage Consumed Alcoholic Beverages |  | Percentage Used Marijuana or Hashish |  | Percentage Used Hard Drugs[a] |  | Percentage Used Marijuana/Hashish or Hard Drugs[a] |  | Percentage with Fair or Poor Health |  |
|---|---|---|---|---|---|---|---|---|---|---|
|  | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Arrest History at Random Assignment |  |  |  |  |  |  |  |  |  |  |
| Never arrested | 31.6 | -1.3 | 7.1 | -0.8 | 1.4 | -0.3 | 7.4 | -0.8 | 16.5 | -1.4 |
| Ever arrested for nonserious crimes only | 38.2 | 5.9* | 12.8 | 0.0 | 1.5 | 3.2*** | 13.3 | 0.4 | 16.9 | -3.2 |
| Ever arrested for serious crimes[c] | 29.4 | 9.5 | 10.8 | 3.3 | 4.9 | -2.4 | 11.3 | 4.6 | 16.9 | -0.7 |
| (P-value)[c] |  | .050** |  | .636 |  | .008*** |  | .463 |  | .808 |
| Race and Ethnicity |  |  |  |  |  |  |  |  |  |  |
| White non-Hispanic | 40.3 | 1.7 | 8.9 | -1.7 | 3.8 | -0.3 | 9.8 | -1.7 | 18.2 | -4.2** |
| Black non-Hispanic | 28.7 | -0.5 | 8.1 | 1.3 | 0.5 | -0.1 | 8.3 | 1.3 | 16.4 | -1.6 |
| Hispanic | 32.6 | 0.7 | 7.1 | -1.3 | 1.7 | 0.6 | 7.2 | -0.9 | 15.8 | -0.6 |
| Other[d] | 37.8 | -4.7 | 12.0 | -3.2 | 2.0 | 1.7 | 12.3 | -2.3 | 19.5 | -2.5 |
| (P-value)[c] |  | .698 |  | .256 |  | .638 |  | .346 |  | .695 |
| Job Corps Application Date and the New Job Corps Policies |  |  |  |  |  |  |  |  |  |  |
| Prior to 3/1/95 (before ZT) | 35.0 | -0.1 | 6.5 | 3.3** | 1.9 | 0.5 | 7.1 | 3.3* | 15.7 | -1.1 |
| On or after 3/1/95 (after ZT) | 32.7 | 0.1 | 9.0 | -1.3 | 1.7 | 0.0 | 9.3 | -1.1 | 17.4 | -2.4** |
| (P-value)[c] |  | .960 |  | .012** |  | .564 |  | .022** |  | .577 |

SOURCE: 30-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Hard drugs include cocaine powder, crack, speed/uppers/methamphetamines, hallucinogens, and heroin/opium/methadone/downers.

[b] Estimated impacts per program participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of eligible applicants in the program group who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[d] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE H.5

## IMPACTS ON KEY ALCOHOL AND ILLEGAL DRUG USE OUTCOMES IN THE 30 DAYS PRIOR TO THE 48-MONTH INTERVIEW AND ON HEALTH STATUS AT 48 MONTHS, BY SUBGROUP

| | Percentage Consumed Alcoholic Beverages | | Percentage Used Marijuana or Hashish | | Percentage Used Hard Drugs[a] | | Percentage Used Marijuana/Hashish or Hard Drugs[a] | | Percentage with Fair or Poor Health | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Age at Application | | | | | | | | | | |
| 16 and 17 | 34.7 | 1.2 | 9.3 | -0.2 | 1.8 | 0.7 | 9.7 | -0.3 | 16.7 | -0.2 |
| 18 and 19 | 37.9 | -2.0 | 6.9 | -0.6 | 1.8 | -0.1 | 7.3 | -0.8 | 17.4 | -1.7 |
| 20 to 24 | 33.5 | 3.6 | 4.6 | 0.3 | 1.0 | -0.3 | 5.0 | 0.1 | 20.0 | -5.1** |
| (P-value)[c] | | .274 | | .880 | | .359 | | .878 | | .187 |
| Gender | | | | | | | | | | |
| Males | 42.4 | -0.5 | 9.3 | 0.2 | 2.0 | 0.4 | 9.9 | -0.1 | 16.6 | -2.2* |
| Females | 25.1 | 3.2* | 4.3 | -0.8 | 1.0 | -0.1 | 4.5 | -0.7 | 19.7 | -1.4 |
| (P-value)[c] | | .145 | | .484 | | .416 | | .623 | | .656 |
| Residential Designees | | | | | | | | | | |
| Males | 42.6 | -0.1 | 9.5 | -0.1 | 2.1 | 0.5 | 10.2 | -0.3 | 16.6 | -2.3* |
| Females | 25.6 | 4.1* | 4.9 | -0.9 | 1.0 | 0.0 | 5.1 | -1.0 | 19.9 | -1.0 |
| (P-value)[c] | | .115 | | .561 | | .517 | | .654 | | .540 |
| Nonresidential Designees | | | | | | | | | | |
| Males | 39.9 | -2.5 | 6.1 | 3.6 | 0.9 | -1.0 | 6.1 | 3.5 | 16.5 | 0.6 |
| Females | 23.6 | 0.0 | 2.5 | -0.1 | 0.7 | -0.6 | 2.5 | 0.2 | 19.0 | -2.8 |
| (P-value)[c] | | .716 | | .286 | | .684 | | .336 | | .564 |
| Presence of Children at Random Assignment for Females | | | | | | | | | | |
| Had children | 29.9 | 1.0 | 5.0 | -0.4 | 1.4 | -0.6 | 5.2 | 0.1 | 19.8 | -0.9 |
| Had no children | 36.8 | 0.6 | 7.7 | -0.1 | 1.7 | 0.4 | 8.2 | -0.4 | 17.4 | -2.2** |
| (P-value)[c] | | .927 | | .909 | | .230 | | .725 | | .568 |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 44.0 | -0.7 | 11.2 | -5.0 | 4.8 | -7.0*** | 14.3 | -9.0** | 19.3 | -8.7* |
| Had no high school credential | 34.9 | 1.7 | 7.6 | 0.5 | 1.6 | 0.6 | 7.9 | 0.5 | 17.9 | -0.2 |
| (P-value)[c] | | .697 | | .153 | | .000*** | | .020** | | .082* |

TABLE H.5 *(continued)*

| Subgroup | Percentage Consumed Alcoholic Beverages |  | Percentage Used Marijuana or Hashish |  | Percentage Used Hard Drugs[a] |  | Percentage Used Marijuana/Hashish or Hard Drugs[a] |  | Percentage with Fair or Poor Health |  |
|---|---|---|---|---|---|---|---|---|---|---|
|  | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Arrest History at Random Assignment |  |  |  |  |  |  |  |  |  |  |
| Never arrested | 33.1 | 0.6 | 6.4 | -0.8 | 1.3 | -0.2 | 6.7 | -0.9 | 18.2 | -2.7 |
| Ever arrested for nonserious crimes only | 41.2 | 4.8 | 10.0 | 1.5 | 1.6 | 3.0*** | 10.8 | 1.0 | 16.3 | 0.7 |
| Ever arrested for serious crimes[c] | 38.4 | 0.6 | 10.0 | 2.3 | 4.2 | -1.1 | 11.0 | 2.8 | 15.4 | 4.6 |
| (P-value)[c] |  | .504 |  | .411 |  | .014** |  | .470 |  | .162 |
| Race and Ethnicity |  |  |  |  |  |  |  |  |  |  |
| White non-Hispanic | 45.8 | 1.2 | 8.3 | -1.4 | 3.5 | -0.1 | 9.2 | -2.3 | 20.0 | -5.2** |
| Black non-Hispanic | 29.6 | 1.8 | 7.0 | 0.9 | 0.5 | -0.1 | 7.1 | 1.1 | 17.2 | -1.0 |
| Hispanic | 36.0 | -1.8 | 6.8 | -1.5 | 1.5 | 1.0 | 7.2 | -1.7 | 15.8 | 2.2 |
| Other[d] | 33.6 | -2.5 | 6.6 | -0.4 | 2.0 | 1.2 | 7.5 | -0.1 | 19.2 | -4.8 |
| (P-value)[c] |  | .646 |  | .434 |  | .544 |  | .211 |  | .090* |
| Job Corps Application Date and the New Job Corps Policies |  |  |  |  |  |  |  |  |  |  |
| Prior to 3/1/95 (before ZT) | 34.9 | 3.7 | 5.7 | 2.5* | 1.8 | 0.6 | 6.3 | 2.1 | 15.1 | 2.4 |
| On or after 3/1/95 (after ZT) | 35.6 | -0.1 | 7.7 | -1.0 | 1.6 | 0.1 | 8.1 | -1.1 | 18.6 | -3.0*** |
| (P-value)[c] |  | .214 |  | .028** |  | .514 |  | .057* |  | .021** |

SOURCE: 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Hard drugs include cocaine powder, crack, speed/uppers/methamphetamines, hallucinogens, and heroin/opium/methadone/downers.

[b] Estimated impacts per program participant are measured as the difference between the weighted means for program and control group members divided by the difference between the proportion of eligible applicants in the program group who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[d] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

# APPENDIX I

## SUPPLEMENTARY TABLES TO CHAPTER VII: IMPACTS ON FAMILY FORMATION AND MOBILITY

## TABLE I.1

## IMPACTS ON CHILD CARE UTILIZATION FOR MALES, BY TYPE OF ARRANGEMENT AND YEAR

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Percentage Ever Used Type of Arrangement During the 48 Months After Random Assignment** | | | | | | |
| Relative | 32.8 | 32.8 | 0.0 | 31.8 | 0.0 | 0.0 |
| Other parent | 29.7 | 29.5 | 0.2 | 28.9 | 0.3 | 1.1 |
| Grandparent | 7.5 | 7.8 | -0.4 | 7.3 | -0.5 | -6.2 |
| Other relative | 1.9 | 2.1 | -0.1 | 1.9 | -0.2 | -8.5 |
| Nonrelative | 3.1 | 3.1 | 0.0 | 3.1 | 0.0 | 1.4 |
| Paid | 2.3 | 2.5 | -0.2 | 2.3 | -0.3 | -10.7 |
| Unpaid | 1.0 | 0.8 | 0.2 | 1.0 | 0.3 | 51.2 |
| Day care center, nursery school, or preschool | 3.9 | 4.1 | -0.1 | 3.6 | -0.2 | -5.0 |
| Kindergarten or elementary school | 0.6 | 0.6 | 0.0 | 0.5 | 0.0 | -8.0 |
| On site at education program or job | 0.2 | 0.0 | 0.1 | 0.2 | 0.2 | 387.2 |
| **Percentage Ever Used Type of Arrangement in Year 1** | | | | | | |
| Relative | 10.9 | 10.0 | 0.9 | 10.8 | 1.3 | 13.3 |
| Other parent | 9.0 | 8.6 | 0.4 | 9.0 | 0.6 | 6.8 |
| Grandparent | 2.4 | 1.8 | 0.6* | 2.2 | 0.9* | 63.3 |
| Other relative | 0.3 | 0.5 | -0.2 | 0.3 | -0.2 | -43.6 |
| Nonrelative | 0.5 | 0.6 | -0.1 | 0.5 | -0.2 | -24.5 |
| Paid | 0.3 | 0.6 | -0.2 | 0.4 | -0.3 | -46.2 |
| Unpaid | 0.2 | 0.1 | 0.1 | 0.1 | 0.2 | -247.6 |
| Day care center, nursery school, or preschool | 0.3 | 0.6 | -0.3* | 0.3 | -0.4* | -57.3 |
| Kindergarten or elementary school | 0.0 | 0.0 | 0.0 | 0.1 | 0.1 | 0.0 |
| On site at education program or job | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -19.6 |
| **Percentage Ever Used Type of Arrangement in Year 4** | | | | | | |
| Relative | 23.3 | 23.1 | 0.2 | 22.2 | 0.3 | 1.3 |
| Other parent | 19.6 | 19.4 | 0.2 | 18.9 | 0.3 | 1.5 |
| Grandparent | 3.9 | 4.2 | -0.3 | 3.7 | -0.3 | -8.4 |
| Other relative | 1.1 | 0.9 | 0.2 | 1.0 | 0.2 | 28.3 |
| Nonrelative | 1.7 | 1.8 | -0.1 | 1.8 | -0.1 | -5.6 |
| Paid | 1.3 | 1.4 | -0.1 | 1.4 | -0.1 | -8.4 |
| Unpaid | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 3.4 |
| Day care center, nursery school, or preschool | 3.0 | 2.8 | 0.2 | 2.8 | 0.2 | 9.9 |
| Kindergarten or elementary school | 0.4 | 0.4 | 0.0 | 0.4 | 0.0 | 10.3 |
| On site at education program or job | 0.1 | 0.0 | 0.1 | 0.1 | 0.1 | |
| **Sample Size** | **3,741** | **2,787** | **6,528** | **2,799** | | |

SOURCE: Baseline and 12-month, 30-month, and 48-month follow-up interview data for those who completed 48-month interviews.

TABLE I.1 *(continued)*

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NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE I.2

## IMPACTS ON CHILD CARE UTILIZATION FOR FEMALES WITHOUT CHILDREN AT RANDOM ASSIGNMENT, BY TYPE OF ARRANGEMENT AND YEAR

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Percentage Ever Used Type of Arrangement During the 48 Months After Random Assignment** | | | | | | |
| Relative | 32.3 | 28.9 | 3.4** | 30.9 | 4.8** | 18.5 |
| Other parent | 12.7 | 11.0 | 1.7 | 12.3 | 2.4 | 23.9 |
| Grandparent | 19.9 | 18.4 | 1.5 | 18.8 | 2.2 | 13.1 |
| Other relative | 10.9 | 8.7 | 2.2** | 10.0 | 3.1** | 45.4 |
| Nonrelative | 10.0 | 7.6 | 2.4** | 10.3 | 3.4** | 48.9 |
| Paid | 7.9 | 5.9 | 2.0** | 8.4 | 2.8** | 50.6 |
| Unpaid | 2.7 | 2.2 | 0.5 | 2.6 | 0.7 | 38.6 |
| Day care center, nursery school, or preschool | 11.3 | 11.5 | -0.2 | 10.7 | -0.2 | -2.1 |
| Kindergarten or elementary school | 0.3 | 0.1 | 0.3 | 0.3 | 0.4 | -387.7 |
| On site at education program or job | 1.1 | 0.5 | 0.6* | 1.2 | 0.8* | 251.7 |
| **Percentage Ever Used Type of Arrangement in Year 1** | | | | | | |
| Relative | 3.1 | 2.9 | 0.2 | 2.4 | 0.3 | 12.8 |
| Other parent | 1.0 | 0.8 | 0.1 | 0.8 | 0.2 | 38.1 |
| Grandparent | 1.7 | 1.4 | 0.3 | 1.4 | 0.4 | 33.9 |
| Other relative | 0.6 | 0.8 | -0.2 | 0.4 | -0.3 | -42.2 |
| Nonrelative | 0.3 | 0.7 | -0.5** | 0.1 | -0.7** | -85.3 |
| Paid | 0.1 | 0.6 | -0.5** | 0.0 | -0.7** | -93.3 |
| Unpaid | 0.1 | 0.2 | 0.0 | 0.1 | 0.0 | -15.6 |
| Day care center, nursery school, or preschool | 0.6 | 0.8 | -0.1 | 0.3 | -0.2 | -42.7 |
| Kindergarten or elementary school | 0.1 | 0.0 | 0.1 | 0.0 | 0.2 | -99.7 |
| On site at education program or job | 0.2 | 0.1 | 0.1 | 0.1 | 0.1 | -3442.8 |
| **Percentage Ever Used Type of Arrangement in Year 4** | | | | | | |
| Relative | 24.8 | 21.9 | 2.9* | 23.1 | 4.1* | 21.6 |
| Other parent | 7.6 | 6.6 | 1.0 | 7.2 | 1.4 | 24.2 |
| Grandparent | 13.4 | 11.9 | 1.4 | 12.6 | 2.0 | 19.2 |
| Other relative | 6.9 | 6.0 | 0.9 | 5.8 | 1.2 | 26.8 |
| Nonrelative | 6.7 | 4.3 | 2.4*** | 7.2 | 3.4*** | 88.8 |
| Paid | 5.4 | 3.2 | 2.2*** | 6.0 | 3.1*** | 110.3 |
| Unpaid | 1.4 | 1.2 | 0.3 | 1.4 | 0.4 | 41.6 |
| Day care center, nursery school, or preschool | 9.3 | 10.1 | -0.8 | 9.2 | -1.2 | -11.4 |
| Kindergarten or elementary school | 0.1 | 0.1 | 0.1 | 0.2 | 0.1 | 128.9 |
| On site at education program or job | 0.5 | 0.2 | 0.3 | 0.5 | 0.4 | 377.2 |
| **Sample Size** | **2,060** | **1,146** | **3,206** | **1,477** | | |

SOURCE: Baseline and 12-month, 30-month, and 48-month follow-up interview data for those who completed 48-month interviews.

TABLE I.2 *(continued)*

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NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE I.3

## IMPACTS ON CHILD CARE UTILIZATION FOR FEMALES WITH CHILDREN AT RANDOM ASSIGNMENT, BY TYPE OF ARRANGEMENT AND YEAR

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Percentage Ever Used Type of Arrangement During the 48 Months After Random Assignment** | | | | | | |
| Relative | 79.3 | 76.9 | 2.5 | 80.9 | 4.0 | 5.1 |
| Other parent | 30.2 | 28.5 | 1.7 | 33.1 | 2.8 | 9.1 |
| Grandparent | 59.9 | 52.9 | 7.0*** | 64.6 | 11.3 *** | 21.2 |
| Other relative | 29.3 | 32.3 | -3.0 | 26.9 | -4.9 | -15.3 |
| Nonrelative | 26.4 | 27.1 | -0.7 | 26.6 | -1.2 | -4.2 |
| Paid | 20.4 | 21.2 | -0.8 | 20.9 | -1.3 | -5.8 |
| Unpaid | 8.4 | 9.4 | -1.0 | 8.9 | -1.6 | -15.1 |
| Day care center, nursery school, or preschool | 45.0 | 41.1 | 3.9 | 43.2 | 6.3 | 17.0 |
| Kindergarten or elementary school | 14.4 | 15.2 | -0.9 | 13.4 | -1.4 | -9.5 |
| On site at education program or job | 6.3 | 4.0 | 2.3* | 6.8 | 3.6 * | 116.1 |
| **Percentage Ever Used Type of Arrangement in Year 1** | | | | | | |
| Relative | 55.9 | 46.8 | 9.1*** | 64.3 | 14.7*** | 29.7 |
| Other parent | 13.3 | 12.3 | 1.0 | 16.1 | 1.6 | 11.1 |
| Grandparent | 38.6 | 25.9 | 12.7*** | 48.1 | 20.4*** | 73.7 |
| Other relative | 12.9 | 12.6 | 0.3 | 12.7 | 0.5 | 4.4 |
| Nonrelative | 10.0 | 8.3 | 1.6 | 9.5 | 2.7 | 38.8 |
| Paid | 6.8 | 6.6 | 0.2 | 6.4 | 0.3 | 4.4 |
| Unpaid | 3.5 | 2.0 | 1.5* | 3.7 | 2.5* | 194.5 |
| Day care center, nursery school, or preschool | 21.9 | 19.6 | 2.4 | 22.5 | 3.8 | 20.5 |
| Kindergarten or elementary school | 1.1 | 2.6 | -1.4** | 0.8 | -2.3** | -74.3 |
| On site at education program or job | 3.5 | 1.3 | 2.2** | 4.3 | 3.6** | 507.1 |
| **Percentage Ever Used Type of Arrangement in Year 4** | | | | | | |
| Relative | 47.0 | 46.0 | 1.0 | 46.8 | 1.6 | 3.6 |
| Other parent | 12.8 | 11.6 | 1.3 | 13.8 | 2.0 | 17.4 |
| Grandparent | 27.1 | 24.2 | 2.9 | 25.3 | 4.7 | 23.0 |
| Other relative | 11.8 | 13.8 | -2.0 | 11.5 | -3.2 | -21.7 |
| Nonrelative | 11.3 | 11.8 | -0.5 | 11.4 | -0.8 | -6.5 |
| Paid | 9.6 | 9.5 | 0.1 | 9.7 | 0.2 | 1.9 |
| Unpaid | 2.1 | 3.4 | -1.3 | 2.2 | -2.1 | -48.3 |
| Day care center, nursery school, or preschool | 24.3 | 22.6 | 1.7 | 23.3 | 2.7 | 13.1 |
| Kindergarten or elementary school | 10.4 | 11.4 | -0.9 | 9.5 | -1.5 | -13.7 |
| On site at education program or job | 0.9 | 1.1 | -0.2 | 0.6 | -0.3 | -31.9 |
| **Sample Size** | **1,005** | **538** | **1,543** | **637** | | |

SOURCE: Baseline and 12-month, 30-month, and 48-month follow-up interview data for those who completed 48-month interviews.

TABLE I.3 *(continued)*

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NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE I.4

## IMPACTS ON HOURS USED CHILD CARE UTILIZATION FOR MALES AND FOR FEMALES WITH AND WITHOUT CHILDREN AT RANDOM ASSIGNMENT, BY TYPE OF ARRANGEMENT

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Total Sample** | | | | | | |
| Average Hours Ever Used Type of Arrangement During the 48 Months After Random Assignment | | | | | | |
| Relative | 4.2 | 3.8 | 0.4** | 4.0 | 0.5** | 15.5 |
| Other parent | 2.4 | 2.3 | 0.0 | 2.3 | 0.1 | 2.4 |
| Grandparent | 1.3 | 1.1 | 0.3*** | 1.3 | 0.4*** | 42.8 |
| Other relative | 0.5 | 0.4 | 0.1 | 0.4 | 0.1 | 35.2 |
| Nonrelative | 0.5 | 0.5 | 0.0 | 0.4 | -0.1 | -11.2 |
| Paid | 0.4 | 0.4 | 0.0 | 0.3 | 0.0 | -10.6 |
| Unpaid | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | -11.8 |
| Day care center, nursery school, or preschool | 1.0 | 0.9 | 0.1** | 0.9 | 0.2** | 31.2 |
| Kindergarten or elementary school | 0.2 | 0.1 | 0.0 | 0.1 | 0.0 | 37.7 |
| On site at education program or job | 0.06 | 0.03 | 0.02* | 0.06 | 0.03 | 126.8 |
| **Males** | | | | | | |
| Average Hours Ever Used Type of Arrangement | | | | | | |
| Relative | 4.0 | 3.9 | 0.1 | 3.7 | 0.2 | 4.2 |
| Other parent | 3.3 | 3.3 | 0.1 | 3.2 | 0.1 | 3.2 |
| Grandparent | 0.6 | 0.5 | 0.0 | 0.5 | 0.0 | 3.4 |
| Other relative | 0.1 | 0.1 | 0.0 | 0.1 | 0.1 | 81.1 |
| Nonrelative | 0.2 | 0.3 | -0.1* | 0.2 | -0.1* | -33.4 |
| Paid | 0.1 | 0.2 | -0.1** | 0.1 | -0.1** | -44.0 |
| Unpaid | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | 16.8 |
| Day care center, nursery school, or preschool | 0.3 | 0.3 | 0.0 | 0.3 | 0.0 | -9.9 |
| Kindergarten or elementary school | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | 2.5 |
| On site at education program or job | 0.01 | 0.0 | 0.0 | 0.01 | 0.01 | 824.7 |
| **Females Without Children** | | | | | | |
| Average Hours Ever Used Type of Arrangement | | | | | | |
| Relative | 2.3 | 2.0 | 0.3* | 2.2 | 0.5* | 28.4 |
| Other parent | 0.6 | 0.5 | 0.1 | 0.6 | 0.2 | 50.5 |
| Grandparent | 1.1 | 1.0 | 0.1 | 1.1 | 0.2 | 16.5 |
| Other relative | 0.6 | 0.5 | 0.1 | 0.5 | 0.1 | 40.1 |
| Nonrelative | 0.5 | 0.5 | 0.0 | 0.5 | 0.0 | 4.3 |
| Paid | 0.4 | 0.4 | 0.0 | 0.4 | 0.1 | 14.9 |
| Unpaid | 0.1 | 0.1 | 0.0 | 0.1 | 0.0 | -32.6 |
| Day care center, nursery school, or preschool | 0.8 | 0.8 | 0.0 | 0.7 | 0.0 | -0.4 |
| Kindergarten or elementary school | 0.0 | 0.0 | 0.0 | 0.0 | 0.0 | -207.4 |
| On site at education program or job | 0.04 | 0.01 | 0.03* | 0.04 | 0.04 | 785.8 |

TABLE I.4 *(continued)*

| Outcome Measure | Program Group | Control Group | Estimated Impact for Eligible Applicants[a] | Program Group Job Corps Participants | Estimated Impact for Participants[b] | Percentage Gain from Participation[c] |
|---|---|---|---|---|---|---|
| **Females With Children** | | | | | | |
| Average Hours Ever Used Type of Arrangement | | | | | | |
| Relative | 9.8 | 7.9 | 1.8*** | 10.6 | 3.0*** | 39.2 |
| Other parent | 1.7 | 2.0 | -0.3 | 1.8 | -0.5 | -20.8 |
| Grandparent | 5.8 | 3.9 | 1.9*** | 6.8 | 3.1*** | 83.6 |
| Other relative | 2.0 | 1.9 | 0.1 | 1.8 | 0.2 | 14.3 |
| Nonrelative | 1.7 | 1.8 | -0.1 | 1.8 | -0.1 | -7.1 |
| Paid | 1.4 | 1.4 | 0.0 | 1.4 | 0.0 | -1.8 |
| Unpaid | 0.3 | 0.4 | -0.1 | 0.4 | -0.1 | -22.4 |
| Day care center, nursery school, or preschool | 4.9 | 3.7 | 1.2*** | 4.8 | 1.9*** | 65.2 |
| Kindergarten or elementary school | 1.1 | 1.0 | 0.1 | 1.1 | 0.2 | 25.6 |
| On site at education program or job | 0.3 | 0.3 | 0.1 | 0.4 | 0.1 | 43.2 |
| **Sample Size** | **6,828** | **4,485** | **11,313** | **4,925** | | |

SOURCE: Baseline and 12-month, 30-month, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of the estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts for eligible applicants are measured as the difference between the weighted means for program and control group members.

[b] Estimated impacts for Job Corps participants are measured as the estimated impacts for eligible applicants divided by the difference between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period. Standard errors for these estimates were inflated to account for the estimation error in the Job Corps participation rate and the control group crossover rate.

[c] The percentage gain from participation is measured as the estimated impact for participants divided by the difference between the mean outcome for participants and the estimated impact for participants.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE I.5

## IMPACTS ON KEY FERTILITY, LIVING ARRANGEMENT, AND CHILD CARE OUTCOMES, BY SUBGROUP

| | Percentage Had New Children | | Percentage of Parents Living with All Their Children at 48 Months | | Percentage Living with Their Parents at 48 Months | | Percentage Living with a Partner (Married or Unmarried) at 48 Months | | Average Hours per Week Used Child Care During the 48-Month Period | |
|---|---|---|---|---|---|---|---|---|---|---|
| Subgroup | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Age at Application | | | | | | | | | | |
| 16 and 17 | 39.1 | 1.0 | 65.6 | 3.1 | 37.6 | -2.9 | 27.1 | 1.4 | 3.9 | 0.3 |
| 18 and 19 | 40.6 | 1.2 | 68.6 | -1.0 | 33.6 | -3.7 | 29.4 | 2.7 | 5.4 | 1.0* |
| 20 to 24 | 32.4 | 3.8 | 66.9 | 5.6 | 31.4 | -6.2** | 32.8 | 3.1 | 7.5 | 1.3* |
| (P-value)[b] | | .685 | | .377 | | .673 | | .873 | | .327 |
| Residential Designees | | | | | | | | | | |
| Males | 30.6 | 0.7 | 42.1 | -0.6 | 37.0 | -3.4** | 29.8 | 1.1 | 4.5 | -0.1 |
| Females without children at baseline | 44.4 | 5.1* | 95.1 | -0.7 | 35.0 | -5.7** | 28.9 | 4.2* | 3.1 | 0.7* |
| Females with children at baseline | 53.6 | 1.5 | 76.8 | 3.8 | 18.8 | 3.0 | 31.6 | -0.6 | 14.5 | 4.1** |
| (P-value)[b] | | .406 | | .740 | | .300 | | .537 | | .022** |
| Nonresidential Designees | | | | | | | | | | |
| Males | 37.1 | -3.4 | 37.1 | 10.9 | 45.6 | -12.6** | 30.2 | 1.3 | 5.6 | 1.2 |
| Females without children at baseline | 44.9 | 3.2 | 93.4 | 6.7* | 32.5 | 2.6 | 29.5 | 3.3 | 4.3 | -0.2 |
| Females with children at baseline | 58.9 | -5.0 | 91.3 | 5.5** | 22.1 | -7.9* | 23.9 | 8.6* | 15.6 | 6.2*** |
| (P-value)[b] | | .637 | | .782 | | .198 | | .650 | | .002*** |
| Educational Attainment at Random Assignment | | | | | | | | | | |
| Had high school diploma or GED | 31.2 | 6.2** | 76.4 | -1.3 | 33.4 | -5.6** | 30.9 | 6.6** | 6.7 | 1.3** |
| Had no high school credential | 40.0 | 0.5 | 64.3 | 3.4* | 35.1 | -3.6** | 28.8 | 1.2 | 4.9 | 0.6** |
| (P-value)[b] | | .075* | | .250 | | .602 | | .089* | | .324 |
| Arrest History at Random Assignment | | | | | | | | | | |
| Never arrested | 37.1 | 3.9*** | 71.7 | 1.7 | 36.2 | -5.1*** | 28.3 | 3.5** | 5.3 | 1.0*** |
| Ever arrested for nonserious crimes only | 39.8 | -2.6 | 59.1 | -0.3 | 32.9 | -2.2 | 31.5 | 0.0 | 5.5 | -0.7 |
| Ever arrested for serious crimes | 40.5 | -8.5 | 46.7 | 3.7 | 33.4 | -9.9* | 34.4 | -5.2 | 5.6 | -0.8 |
| (P-value)[b] | | .032** | | .897 | | .439 | | .222 | | .014** |

TABLE I.5 *(continued)*

| Subgroup | Percentage Had New Children | | Percentage of Parents Living with All Their Children at 48 Months | | Percentage Living with Their Parents at 48 Months | | Percentage Living with a Partner (Married or Unmarried) at 48 Months | | Average Hours per Week Used Child Care During the 48-Month Period | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] | Control Group | Estimated Impact per Participant[b] |
| Race and Ethnicity | | | | | | | | | | |
| White non-Hispanic | 33.2 | 2.5 | 67.8 | 4.6 | 26.6 | -1.5 | 41.1 | 2.8 | 4.7 | 0.5 |
| Black non-Hispanic | 40.8 | 0.0 | 63.7 | 3.1 | 36.7 | -4.3** | 20.7 | 1.1 | 5.8 | 0.9** |
| Hispanic | 38.7 | 4.7 | 72.8 | -1.4 | 40.9 | -5.5* | 32.1 | 6.2** | 5.3 | 1.2* |
| Other[c] | 33.7 | 3.7 | 73.4 | 0.5 | 36.1 | -5.4 | 35.6 | -1.9 | 4.6 | -0.6 |
| (P-value)[b] | | .551 | | .707 | | .668 | | .369 | | .334 |
| Job Corps Application Date and the New Job Corps Policies | | | | | | | | | | |
| Prior to 3/1/95 (before ZT) | 38.3 | 1.6 | 68.5 | -1.1 | 31.9 | -1.4 | 30.8 | -0.9 | 5.3 | 0.0 |
| On or after 3/1/95 (after ZT) | 37.7 | 1.7 | 66.5 | 3.5* | 35.5 | -4.7*** | 28.9 | 3.1** | 5.4 | 0.9*** |
| (P-value)[b] | | .945 | | .283 | | .251 | | .163 | | .133 |

SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All estimates were calculated using sample weights to account for the sample and survey designs and interview nonresponse. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline.

[a] Estimated impacts per program participant are measured as the difference between the weighted means for program and control group members divided by the difference between proportion of eligible applicants in the program group who enrolled in Job Corps and the proportion of control group members who enrolled in the program during their three-year restriction period.

[b] Figures are p-values from tests to jointly test for differences in program impacts across levels of the subgroup.

[c] This group includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

Contract No.: K-4279-3-00-80-30
MPR Reference No.: 8140-530

**MATHEMATICA**
Policy Research, Inc.

# National Job Corps Study: Methodological Appendixes on the Impact Analysis

*June 2001*

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This report has been produced under Contract Number K-4279-3-00-80-30 with the U.S. Department of Labor, Employment and Training Administration. The contents of the report do not necessarily reflect the views or policies of the Department of Labor, nor does mention of trade names, commercial products, or organizations imply endorsement of these by the U.S. Government.

# ACKNOWLEDGMENTS

This report reflects the contributions of several people. It was skillfully produced by Cathy Harper and Monica Capizzi-Linder and carefully edited by Walter Brower. John Burghardt provided helpful comments.

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# INTRODUCTION

In a series of appendixes, this report discusses methodological issues related to the 48-month impact analysis for the National Job Corps Study. The appendixes are intended to complement the 48-month impact report (Schochet et al. 2001), which presents impacts of Job Corps on key participant outcomes during the 48 months after random assignment.

This report contains the following five appendixes:

1. **"The 12-, 30-, and 48-Month Interviews."** The outcome measures for the 48-month impact analysis were constructed using follow-up interview data collected 12, 30, and 48 months after random assignment. This appendix provides a detailed discussion of the design of the follow-up interviews and examines response rates.

2. **"The Treatment of Missing Values and Outliers."** This appendix describes our procedure for treating missing values and outliers for the outcome measures used in the 48-month impact analysis.

3. **"The Adjustment for Crossovers."** This brief appendix describes procedures that were used to adjust the impact estimates for the small number of control group members who enrolled in Job Corps during their three-year restriction period and afterwards.

4. **"The Calculation of Sample Weights and Standard Errors."** This appendix discusses the calculation of sample weights used in the 48-month impact analysis to obtain unbiased impact estimates that could be generalized to the study population. The appendix also discusses the calculation of standard errors of the impact estimates.

5. **"Regression-Adjusted Impact Estimates."** This appendix discusses impact estimates obtained using multivariate regression procedures. These regression-adjusted impact estimates are compared to the simple differences-in-means estimates that are presented in the impact report.

# APPENDIX A

## THE 12-, 30-, AND 48-MONTH INTERVIEWS

## A. INTRODUCTION

We obtained estimates over the 48 months after random assignment by comparing the outcomes of program group members (who could enroll in Job Corps) and control group members (who could not). The outcome measures for the analysis were constructed primarily from interview data collected 12, 30, and 48 months after random assignment. This appendix discusses the design and implementation of the follow-up interviews.

Baseline interview data were also used to construct outcome measures covering the period between the random assignment and baseline interview dates. The design and implementation of the baseline interview is discussed in detail in Schochet (1998a). However, we summarize features of the baseline interview because its survey design must be understood if the survey design for the follow-up interview is to be understood.

## B. SURVEY DESIGN

### 1. Design of the Baseline Interview

Baseline interviewing took place between mid-November 1994 and July 1996. Detailed tracking information (contained in program intake forms sent to MPR as part of the random assignment process) was used to help locate youths. The Office of Management and Budget (OMB) approved the offering of a $10 incentive fee to control group members and hard-to-locate program group members to induce them to complete the baseline interview.

After sample members had been randomly assigned, they were contacted by telephone as soon as possible (usually the same day) to increase the proportion of interview respondents who did not know their research status prior to the interview.

At the end of May 1995, we began attempting in-person interviews with sample members not reachable by telephone. We waited until May to conduct these interviews so that enough sample members had been released into the field to make it cost-effective to hire field interviewers. In-person interviews were attempted only with sample members who lived in randomly selected areas when they applied to Job Corps, because it would have been extremely expensive to conduct in-person interviews nationwide.[1] About two-thirds of randomized youths in the study population lived in areas selected for in-person interviewing when they applied to Job Corps.[2]

Sample members in the selected areas were released into the field for in-person interviewing if they could not be reached by telephone within 45 days after random assignment. During the post-45-day period, in-person and telephone interviews were attempted with these youths. However, during this period, neither telephone nor in-person interviews were attempted with youths who lived in the areas not selected for in-person interviewing. Consequently, the sample interviewed within 45 days is a nationally representative *random* sample of eligible applicants who could be interviewed by telephone within 45 days. The sample interviewed after 45 days is a nationally representative *clustered* sample of those who could be reached after 45 days. Both groups combined represent all persons in the study population.[3]

---

[1] In order to define areas for in-person interviewing, we divided the country into three types of areas, on the basis of adjoining groups of counties: (1) those in which about 1,000 Job Corps students resided in 1993 (*superdense* areas), (2) those in which about 600 Job Corps students resided in 1993 (*dense* areas), and (3) those in which about 300 students resided in 1993 (*nondense* areas). The "optimal" number of each type of area to select was calculated to maximize the precision of the impact estimates, subject to the cost of conducting interviews in each type of area and a fixed interview budget. On the basis of this procedure, we randomly selected all 16 superdense areas, 18 of the 29 dense areas, and 29 of the 75 nondense areas for in-person interviewing. All control group members designated for nonresidential slots on the Supplemental ETA-652 form, however, were eligible for in-person interviews to increase the precision of impact estimates for the small nonresidential program component.

[2] The figures for control group members (72 percent) and for program research group members (66.5 percent) differ because sampling rates to the research sample differed for various population subgroups.

[3] We selected the 45-day cutoff after analyzing the cumulative telephone response rates by time

(continued...)

Baseline interviews were no longer attempted for sample members in the selected areas if they did not complete the interview within nine months of random assignment. However, as discussed in the next subsection, these youths were eligible for 12-month follow-up interviews.

### 2. Design of the 12-Month Interview

The 12-month interview was conducted between March 1996 and September 1997. With OMB approval to offer a finder's fee or an incentive payment to hard-to-locate sample members, we offered a $10 inducement to program group members who were not at a Job Corps center and to all control group members. We attempted interviews with youths between 12 and 27 months after their random assignment dates. Interviews completed between months 27 and 30 were 30-month interviews.

The target sample for the 12-month follow-up interview included (1) all sample members selected for in-person interviews at baseline (whether or not they completed a baseline interview), and (2) those not eligible for in-person interviews at baseline who completed the baseline interview by telephone within 45 days after random assignment. Thus, youths who resided in areas not selected for in-person interviews and who did not complete a baseline interview by telephone were not eligible for 12-month (and subsequent) interviews. In addition, we did not attempt follow-up interviews with 77 people selected for the study sample (40 program group and 37 control group members), because these youths were found to have enrolled in Job Corps prior to random assignment. Consistent with our decision to include in the study only youths who had not previously

---

[3](...continued)
since random assignment for the early cohort of sample members. The 45-day cutoff was chosen because telephone response rates increased slowly after this period. Furthermore, we did not want to extend the cutoff date, because we did not want to delay in-person interviewing in the in-person areas.

attended Job Corps, we removed these program readmits from the study sample.[4] Finally, 39 sample members (21 program and 18 control) were confirmed to have died. In total, 14,725 youths (9,017 program and 5,708 control) were released for 12-month interviews.

We completed 12-month interviews with 326 youths (187 program and 139 control) in the in-person areas who had not completed a baseline interview. An abbreviated baseline interview was administered to these "combo" cases at the end of the 12-month interview.

For the 12-month interview, we attempted interviews by telephone first and, if unsuccessful, attempted them in person. In contrast to the in-person interviewing at baseline, there was *no* clustering of in-person interviews in the follow-up interviews. In-person interviewing started in May 1996, after a sufficient number of youths had been released into the field.

### 3. Design of the 30-Month Interview

The 30-month interview was conducted between September 1997 and February 1999. A $10 incentive fee was offered to all those in the target sample. Interviews were attempted with youths until 45 months after their random assignment dates. Interviews completed after then were treated as 48-month interviews.

We attempted a 30-month interview with all sample members who completed either the baseline or the 12-month interview, except for 54 youths who were confirmed to have died since their last interview. In total, 14,671 youths (8,983 program and 5,688 control) were released for 30-month interviews. The 493 respondents to the 30-month interview who completed a baseline interview but not the 12-month interview were asked about their experiences since the baseline interview.

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[4] Because the study design excluded people who had previously enrolled in Job Corps, and because we believed Job Corps staff could identify these youths, Job Corps staff were not supposed to send information on program readmits to MPR for random assignment. However, in fact, staff were not able to identify all readmits, and information was mistakenly sent to MPR for some of these cases. After sample intake ended, we used historical information on center enrollees to identify those in our sample who enrolled in Job Corps prior to random assignment. Because information on the program readmits was sent *prior* to random assignment, there are no differences in the proportion or characteristics of readmits in the program and control groups; thus, we excluded these youths from the study.

As with the 12-month interview, we attempted 30-month interviews by telephone first and, if unsuccessful, attempted them in person to youths in *all* areas. In-person interviewing started in October 1997 and concluded in February 1999.

### 4. Design of the 48-Month Interview

We conducted the 48-month interview between December 1998 and May 2000. Initially, a $10 incentive fee was offered to all those released for interviews, but it was increased to $25 in June 1999 to help boost the response rate.

We attempted a 48-month interview with those who completed any previous interview, with two exceptions. First, we excluded 37 youths who were confirmed to have died since their last interview. Second, to reduce data collection costs, we released only about 93 percent of program group members (8,268 of 8,907) who were eligible for 48-month interviews. These program group members were randomly selected using systematic sampling techniques, where program group members were sorted by residential status, gender, random assignment date, whether the baseline interview was completed within 45 days after random assignment, and age. In total, 13,850 youths (8,268 program and 5,582 control) were released for 48-month interviews. Respondents were asked about their experiences since their previous interview.

We attempted 48-month interviews by telephone first, and attempted interviews in person for youths in all areas who could not be reached by telephone. In-person interviewing started in late April 1999 and concluded in May 2000.

## C. INTERVIEW RESPONSE ISSUES

This section discusses response rates to the baseline and follow-up interviews, the mode of completion of the follow-up interviews, and reasons for noncompletion of the follow-up interviews. First, we summarize results from the baseline interview and the 12- and 30-month follow-up interviews (which were discussed in detail in Schochet 1998a and Schochet 2000). Second, we provide a detailed discussion of results for the 48-month interview.

### 1. The Baseline Interview

The response rate to the baseline interview for sample members in all areas was 93.1 percent. Interviews were completed with 14,327 of the 15,386 youths in the research sample, and most interviews were completed by telephone soon after random assignment. Furthermore, the difference in completion rates between the program and control groups was only 1.5 percentage points (93.8 percent program, 92.3 control). The response rate for sample members in the areas selected for in-person interviewing--the *effective* response rate--was 95.2 percent (95.9 percent program, 94.3 percent control). This is the relevant response rate for the study, because "nonrespondents" in the nonselected areas consisted of both those who would have and those who would not have completed baseline interviews in the post-45-day period if given the chance. Therefore, "true" respondents and nonrespondents can be identified only in the selected areas.

Response rates to the baseline interview were high for all key subgroups (Schochet 1998a). Item nonresponse was infrequent for nearly all data items.

### 2. The 12-Month Interview

We completed 12-month interviews with 13,383 of the 14,725 youths released for 12-month interviews. For those in the in-person areas only, we completed 9,421 of the 10,448 interviews attempted. The effective response rate to the 12-month interview (that is, the response rate in the in-person areas) was 90.2 percent (91.4 percent program, 88.4 percent control).[5,6] Most interview respondents completed the 12-month interview soon after their 12-month release date (Schochet 2000).

The effective response rate to the 12-month interview differed only slightly across key youth subgroups (Schochet 2000). These response rates were calculated using ETA-652 and ETA-652 Supplement data, which are available for *both* interview respondents and nonrespondents, and refer to youth characteristics at the time of application to Job Corps.

It is noteworthy that among those who completed baseline interviews within 45 days after random assignment, the response rate for those who lived in the in-person areas was similar to the rate for those who did not (Schochet 2000). This is an expected result, because the in-person areas were randomly selected.

### 3. The 30-Month Interview

The sample of those who completed 30-month interviews was the primary analysis sample used in the 30-month (short-term) impact report. We completed 30-month interviews with 11,787 of the 14,671 youths released for 30-month interviews. For those in the in-person areas only, we completed

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[5] As mentioned above, the effective response rate is the percentage of sample members in areas selected for in-person interviews at baseline who completed a 12-month interview. This is the relevant response rate for the study, because we did not attempt follow-up interviews with youths who were not selected for in-person interviews at baseline and who did not complete a baseline interview by telephone within 45 days after random assignment.

[6] The response rates exclude the program readmits and youths who died.

8,257 of the 10,405 interviews attempted, which resulted in an effective response rate of 79.4 percent (80.7 percent program, 77.4 percent control).[7] The effective response rate to the 30-month interview was fairly high across all key youth subgroups, and most interview respondents completed the interview soon after it was due to be completed (Schochet 2000).

About 96 percent of those who completed the 30-month interview also completed the 12-month interview. In addition, about 98 percent completed the full baseline interview; the remaining 2 percent were "combo" cases who did not complete the full baseline interview but completed the abbreviated baseline interview as part of the 12-month interview. Thus, complete baseline and follow-up data are available for most youths in the 30-month sample.

### 4. The 48-Month Interview

The sample of those who completed 48-month interviews was the primary analysis sample used in the 48-month impact report. Thus, obtaining sufficiently high response rates to the 48-month interview was crucial for obtaining credible estimates of the impacts of Job Corps on key participant outcomes.

We completed 48-month interviews with 11,313 of the 13,850 youths released for 48-month interviews.[8] In the in-person areas only, we completed 7,940 of the 9,937 interviews attempted. Thus, the effective response rate to the 48-month interview was 79.9 percent (81.5 percent program, 77.8 percent control).[9]

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[7] The response rates exclude the program readmits and those who died.

[8] As noted, 639 randomly selected program group members who were eligible for 48-month interviews were not released for 48-month interviews to reduce data collection costs. Thus, 14,489 youths were eligible for 48-month interviews, although only 13,850 were released.

[9] The response rates exclude the program readmits and those who died.

About 88 percent of the 48-month sample also completed 30-month interviews, and 95 percent completed 12-month interviews. More than 85 percent completed both 12- and 30-month interviews, and only 2 percent completed neither. As with the 30-month sample, baseline interview data are available for everyone in the 48-month sample, because all youths completed either the full baseline interview or an abbreviated baseline interview as part of the 12-month interview.

The response rates differed across some key subgroups, although the differences are small (Table A.1). The response rate was higher for females than for males (85 percent, compared to 76 percent), and the response rate was about six percentage points higher for those who lived in less populated areas than for those who lived in more populated ones. Furthermore, it was slightly higher for (1) those who completed high school, (2) those never arrested or convicted, (3) those who lived with family members, (4) those with health problems, (5) those with children, and (6) likely nonresidential students than for their counterparts. There were few differences by age, race/ ethnicity, or region. Interestingly, the pattern of findings is very similar to that of the 30-month interview.

Because of these subgroup differences in response rates, we adjusted sample weights for the 48-month interview sample to help reduce the potential bias in the impact estimates due to interview nonresponse (see Appendix D). We used these adjusted weights to calculate all impact estimates.

Most interview respondents completed the 48-month interview soon after the 48-month point (Table A.2). We completed the average 48-month interview in month 49.8, and more than 78 percent within 3 months after the 48-month interview release date (that is, before month 51). Less

## TABLE A.1

## EFFECTIVE RESPONSE RATES TO THE 48-MONTH FOLLOW-UP INTERVIEW, BY RESEARCH STATUS AND KEY SUBGROUP

| | Effective Response Rate | | |
|---|---|---|---|
| Subgroup | Program Group | Control Group | Combined Sample |
| Full Sample | 81.5 | 77.8 | 79.9 |
| **Demographic Characteristics** | | | |
| Gender | | | |
| Male | 78.2 | 73.7 | 76.2 |
| Female | 85.6 | 84.6 | 85.2 |
| Age at Application | | | |
| 16 to 17 | 81.4 | 79.2 | 80.4 |
| 18 to 19 | 81.9 | 77.3 | 80.0 |
| 20 to 21 | 81.0 | 76.8 | 79.2 |
| 22 to 24 | 81.1 | 75.6 | 78.9 |
| Race/Ethnicity | | | |
| White, non-Hispanic | 82.4 | 81.4 | 82.0 |
| Black, non-Hispanic | 83.7 | 80.5 | 82.4 |
| Hispanic | 80.1 | 76.4 | 78.5 |
| Other | 80.9 | 79.2 | 80.2 |
| Region | | | |
| 1 | 79.7 | 77.1 | 78.6 |
| 2 | 78.8 | 67.6 | 73.8 |
| 3 | 81.1 | 76.6 | 79.1 |
| 4 | 83.1 | 81.2 | 82.3 |
| 5 | 80.9 | 77.5 | 79.5 |
| 6 | 81.9 | 78.3 | 80.4 |
| 7/8 | 84.4 | 84.3 | 84.4 |
| 9 | 80.1 | 73.8 | 77.3 |
| 10 | 76.7 | 78.5 | 77.5 |
| Size of City of Residence | | | |
| Less than 2,500 | 84.3 | 84.2 | 84.2 |
| 2,500 to 10,000 | 86.4 | 81.8 | 84.5 |
| 10,000 to 50,000 | 81.2 | 79.2 | 80.4 |
| 50,000 to 250,000 | 80.4 | 77.0 | 79.0 |
| 250,000 or more | 81.0 | 76.3 | 79.0 |
| PMSA or MSA Residence Status | | | |
| In PMSA | 79.8 | 73.3 | 77.1 |
| In MSA | 82.4 | 80.4 | 81.5 |
| In neither | 84.4 | 85.1 | 84.7 |

TABLE A.1 *(continued)*

| | Effective Response Rate | | |
|---|---|---|---|
| Subgroup | Program Group | Control Group | Combined Sample |
| Density of Area of Residence | | | |
| Superdense | 80.8 | 75.4 | 78.6 |
| Dense | 80.7 | 78.8 | 79.9 |
| Nondense | 83.8 | 81.8 | 83.0 |
| Lived in Areas with a Large Concentration of Nonresidential Females | | | |
| Yes | 81.4 | 78.7 | 80.2 |
| No | 81.5 | 77.0 | 79.7 |
| Legal U.S. Resident | | | |
| Yes | 81.4 | 77.9 | 80.0 |
| No | 84.0 | 68.6 | 78.0 |
| Job Corps Application Date | | | |
| 11/94 to 2/95 | 81.7 | 77.9 | 80.2 |
| 3/95 to 6/95 | 83.4 | 80.5 | 82.1 |
| 7/95 to 9/95 | 81.8 | 77.2 | 79.8 |
| 10/95 to 12/95 | 78.1 | 74.7 | 76.7 |
| **Fertility and Family Status** | | | |
| Fertility | | | |
| Had dependents | 84.1 | 85.5 | 84.7 |
| Had no dependents | 80.9 | 76.1 | 78.9 |
| Family Status | | | |
| Family head | 83.1 | 80.7 | 82.1 |
| Family member | 81.9 | 78.9 | 80.7 |
| Unrelated individuals | 79.0 | 73.2 | 76.6 |
| **Education** | | | |
| Completed the 12th grade | 84.4 | 79.0 | 82.2 |
| Did not complete the 12th grade | 80.6 | 77.6 | 79.3 |
| **Welfare Dependence** | | | |
| Public Assistance | | | |
| Received AFDC | 82.9 | 81.0 | 82.1 |
| Received other assistance | 80.4 | 79.9 | 80.2 |
| Did not receive | 81.0 | 75.7 | 78.7 |
| **Health** | | | |
| Had Any Health Conditions That Were Being Treated | | | |
| Yes | 88.3 | 83.3 | 86.3 |
| No | 81.8 | 78.2 | 80.3 |

TABLE A.1 *(continued)*

| Subgroup | Effective Response Rate | | |
|---|---|---|---|
| | Program Group | Control Group | Combined Sample |
| **Crime** | | | |
| Arrests | | | |
| Arrested in past three years | 80.6 | 73.8 | 77.6 |
| Not arrested in past three years | 81.6 | 78.6 | 80.3 |
| Convictions | | | |
| Ever convicted or adjudged delinquent | 78.2 | 72.7 | 75.8 |
| Never convicted or adjudged delinquent | 81.6 | 78.2 | 80.2 |
| **Anticipated Program Enrollment Information** | | | |
| Residential Designation Status | | | |
| Resident | 81.1 | 76.6 | 79.2 |
| Nonresident | 82.9 | 82.1 | 82.6 |
| CCC/Contract Center Designation[a] | | | |
| CCC | 82.1 | 78.1 | 80.4 |
| Contract center | 81.3 | 78.6 | 80.2 |
| Performance Level of Designated Center[a] | | | |
| High or medium-high | 81.2 | 77.5 | 79.7 |
| Medium-low or low | 81.6 | 79.4 | 80.6 |
| Size of Designated Center[a] | | | |
| Large or medium-large | 80.7 | 77.1 | 79.2 |
| Medium-small or small | 81.8 | 79.3 | 80.8 |
| **Sample Size** | **5,725** | **4,212** | **9,937** |

SOURCE: ETA-652 and ETA-652 Supplement data.

NOTE: 1. The effective response rate is the response rate for those sample members who were eligible for a baseline interview after 45 days after random assignment. These are youths who lived in randomly selected (in-person) areas at application to Job Corps.

2. The following cases in the in-person areas were excluded from the calculations: (1) 97 cases (43 control group and 54 program group members) who were confirmed to have died since their previous interview, (2) 63 cases (31 control and 32 program) who were determined to have enrolled in Job Corps prior to random assignment, and (3) 443 randomly selected program group members who were eligible for 48-month interviews but, in an effort to reduce data collection costs, were not released for 48-month interviews.

[a] Figures are obtained using data on OA counselor projections about the centers that youths were likely to attend.

TABLE A.2

## DISTRIBUTION OF THE NUMBER OF MONTHS BETWEEN 48 MONTHS AFTER RANDOM ASSIGNMENT AND COMPLETION OF THE 48-MONTH INTERVIEW FOR THOSE IN THE IN-PERSON AREAS, BY RESEARCH STATUS

(Percentages)

| Number of Months | Program Group | Control Group | Combined Sample |
|---|---|---|---|
| -3 to 0[a] | 11.6 | 13.4 | 12.4 |
| 0 to .5 | 28.2 | 29.2 | 28.6 |
| .5 to 1 | 14.4 | 14.7 | 14.5 |
| 1 to 2 | 13.7 | 13.2 | 13.5 |
| 2 to 3 | 9.7 | 9.2 | 9.5 |
| 3 to 4 | 6.6 | 6.6 | 6.6 |
| 4 to 5 | 4.9 | 4.5 | 4.7 |
| 5 to 6 | 3.6 | 3.4 | 3.5 |
| 6 to 12 | 6.4 | 5.3 | 5.9 |
| 12 or More | 0.9 | 0.6 | 0.8 |
| Average Number of Months | 1.8 | 1.7 | 1.8 |
| **Number of Respondents to the 48-Month Interview** | **4,661** | **3,273** | **7,934** |

SOURCE: 48-month follow-up interview data.

NOTE: The in-person areas are randomly selected areas in which youths were eligible for baseline interviews after 45 days after random assignment. Youths not in the in-person areas who did not complete baseline interviews within the 45-day period were not eligible for follow-up interviews.

[a] Youths in the in-person areas who did not complete the 30-month interview within 45 months after random assignment but who were located before 48 months after random assignment were administered the 48-month interview.

than 7 percent of interviews were completed more than six months after the release date (that is, after month 54). The distributions of completion times were similar for program and control group members. The fact that most interviews were conducted quickly and that most 48-month respondents also completed 12- and 30-month interviews suggests that recall error did not have a large effect on item responses and that recall error did not differ substantially across sample members.

About 85 percent of interviews were completed by telephone in MPR's phone center (Table A.3). About 15 percent were conducted in the field (8.5 percent in person, 5.5 percent when the field interviewer had the youth call the MPR phone center, and 1 percent when the field interviewer called the youth).[10] About 1 percent were completed while the respondent was at a Job Corps center. A higher percentage of males than females completed interviews in the field (about 18 percent, compared to 11 percent) and a smaller percentage of males completed interviews by telephone (about 82 percent, compared to 89 percent). The figures are similar for program and control group members.

As expected, the proportion of interviews completed in the field was higher for the 48-month interview (15 percent) than for the 30-month interview (13 percent) and the 12-month interview (6.5 percent), because it became increasingly difficult to locate youths by phone.

Most interview nonrespondents were youths who could not be located, although some were youths who were located but refused to complete the interview (Table A.4). Our survey staff were unable to locate about 74 percent of program group nonrespondents and 73 percent of control group

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[10] We conducted (1) 45 interviews with youths who were living at a Job Corps center, (2) 12 interviews with youths who were living in a school or college, (2) 522 interviews with youths in jail (297 program and 223 control), (3) 29 interviews with youths living in halfway houses or residential treatment centers, (4) 79 interviews with youths in the military, (5) 25 interviews with youths in a group home, and (6) 16 interviews with homeless youths. The rest were conducted at a private residence.

## TABLE A.3

## INTERVIEW MODE FOR CASES WHO COMPLETED THE 48-MONTH INTERVIEW, BY RESEARCH STATUS AND GENDER

(Percentages)

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Interview Mode | Males | Females | Total | Male | Females | Total |
| Telephone Center | 80.7 | 88.3 | 84.1 | 82.2 | 89.5 | 84.9 |
| In the Field | 19.3 | 11.7 | 15.8 | 17.8 | 10.4 | 15.0 |
| Interviewer called youth | 1.8 | 1.0 | 1.5 | 1.2 | 1.2 | 1.2 |
| Interviewer had youth use a cell phone to call the phone center | 10.0 | 7.5 | 8.9 | 9.0 | 7.0 | 8.3 |
| In person | 7.4 | 3.2 | 5.5 | 7.6 | 2.2 | 5.6 |
| Interview Conducted While Respondent Was at a Job Corps Center[a] | 0.6 | 0.8 | 0.7 | 1.4 | 1.2 | 1.3 |
| **Number of Respondents to the 48-Month Interview** | **3,741** | **3,087** | **6,828** | **2,787** | **1,698** | **4,485** |

SOURCE: 48-month follow-up interview data.

[a] Interviews conducted at Job Corps are counted as having been conducted by telephone or in the field (that is, this category is not exclusive of the other categories).

## TABLE A.4

## REASONS FOR NONCOMPLETION OF THE 48-MONTH INTERVIEW, BY RESEARCH STATUS AND GENDER
(Percentages)

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Reasons for Noncompletion | Males | Females | Total | Male | Females | Total |
| Unable to Locate | 75.6 | 71.6 | 74.3 | 73.7 | 70.4 | 73.0 |
| Refusal | 16.5 | 23.6 | 18.8 | 17.6 | 24.3 | 19.1 |
| Incarcerated and Unavailable | 3.2 | 1.1 | 2.5 | 4.9 | 1.2 | 4.1 |
| In Military and Unavailable | 2.0 | 0.2 | 1.4 | 1.4 | 0.8 | 1.3 |
| Break-Off or Partial Interview | 2.2 | 2.0 | 2.1 | 1.3 | 1.7 | 1.4 |
| Other | 0.5 | 1.5 | 0.9 | 1.1 | 1.7 | 1.2 |
| **Number of Nonrespondents to the 48-Month Interview** | **960** | **457** | **1,417** | **837** | **243** | **1,080** |

SOURCE: 48-month follow-up interview data.

nonrespondents. The refusal rate was about 19 percent for both research groups, but it was higher for females than males (24 percent, compared to 17 percent). Not surprisingly, the refusal rate at 48 months (19 percent) was higher than it was at 30 months (15 percent) and at 12 months (6.5 percent). Among male nonrespondents, about 3 percent of program group members and 5 percent of control group members did not complete the interview because they were in jail and unavailable, and about 2 percent were in the military and unavailable. Finally, an additional 2 percent of nonrespondents broke off the interview or completed only part of it.

# APPENDIX B

# THE TREATMENT OF MISSING VALUES AND OUTLIERS

## A. INTRODUCTION

We constructed three categories of outcome measures for the 48-month impact analysis: (1) education and training in Job Corps and elsewhere; (2) employment and earnings; and (3) nonlabor market outcomes, including the receipt of public assistance benefits, involvement with the criminal justice system, use of alcohol and illegal drugs, health, fertility, custodial responsibility for children, marital status, living arrangements, child care, and mobility. The 48-month impact report describes the specific outcome measures used in the analysis, our reasons for selecting these measures, and our basic procedure for constructing them. This appendix discusses in more detail the construction of key outcome measures and examines the prevalence of missing values and outliers.

## B. THE PREVALENCE OF MISSING VALUES

Table B.1 displays the proportion of the 48-month sample with nonmissing values for selected outcome measures. The figures are presented separately for program and control group members, and are presented for the full sample and by gender.

Data item nonresponse was uncommon for most outcome measures used in the 48-month impact analysis. Indicators of the occurrence of key events are rarely missing. For example, item nonresponse was typically less than 3 percent for indicators of (1) participation in Job Corps and other education and training programs (such as GED, high school, or vocational schools); (2) educational attainment (such as the receipt of GED and vocational trade certificates and highest grade completed); (3) employment and characteristics of the most recent job; (4) the receipt of various forms of public assistance benefits; (5) arrests, arrest charges, convictions, and incarcerations for convictions; (6) alcohol and various types of illegal drug use; (7) health status; (8) fertility; (9) child care; and (10) marital status and living arrangements.

## TABLE B.1

## DATA ITEM RESPONSE FOR KEY OUTCOME MEASURES USED IN THE 48-MONTH IMPACT ANALYSIS, BY RESEARCH STATUS AND GENDER

(Percentages)

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Males | Females | Total | Males | Females | Total |
| **Job Corps Experiences** | | | | | | |
| Enrolled in a Job Corps Center | | | | | | |
| All months | 98.4 | 99.4 | 98.8 | NA | NA | NA |
| Quarter 1 | 97.2 | 98.7 | 97.9 | NA | NA | NA |
| Quarter 5 | 98.6 | 99.1 | 98.8 | NA | NA | NA |
| Quarter 16 | 100.0 | 100.0 | 100.0 | NA | NA | NA |
| Months Between Random Assignment and Center Enrollment[a] | 95.0 | 96.8 | 95.8 | NA | NA | NA |
| Months Enrolled[a] | 92.0 | 94.2 | 92.9 | NA | NA | NA |
| Months Between Date Left Job Corps and the 48-Month Interview[a] | 94.9 | 96.3 | 95.5 | NA | NA | NA |
| Participated in Academic Classes or Vocational Training[a] | 97.1 | 98.6 | 97.8 | NA | NA | NA |
| Total Hours in Academic Classes and Vocational Training[a] | 82.2 | 85.6 | 83.7 | NA | NA | NA |
| Took Academic Classes[a] | 97.2 | 98.6 | 97.9 | NA | NA | NA |
| Total Hours in Academic Classes[a] | 84.3 | 87.9 | 85.9 | NA | NA | NA |
| Took Vocational Training[a] | 97.2 | 98.6 | 97.8 | NA | NA | NA |
| Total Hours in Vocational Training[a] | 84.5 | 87.9 | 86.1 | NA | NA | NA |
| Participation in Other Job Corps[b] Activities | | | | | | |
| World of Work | 92.6 | 95.4 | 93.8 | NA | NA | NA |
| Progress/Performance Evaluation Panels | 92.8 | 95.8 | 94.1 | NA | NA | NA |
| Health Classes | 92.8 | 95.5 | 94.0 | NA | NA | NA |
| Parenting Skills Classes | 93.2 | 96.3 | 94.6 | NA | NA | NA |
| Social Skills Training | 92.3 | 94.7 | 93.3 | NA | NA | NA |
| Cultural Awareness Classes | 92.3 | 94.9 | 93.5 | NA | NA | NA |
| Alcohol and Other Drugs of Abuse Program | 93.3 | 96.4 | 94.6 | NA | NA | NA |
| **Education and Training in Job Corps and Elsewhere** | | | | | | |
| Enrolled in a Program, by Period | | | | | | |
| Ever during the 48 months | 96.2 | 97.8 | 96.9 | 95.5 | 97.8 | 96.4 |
| Quarter 1 | 96.0 | 97.4 | 96.6 | 94.2 | 96.7 | 95.2 |
| Quarter 5 | 96.8 | 98.3 | 97.5 | 97.9 | 98.8 | 98.2 |
| Quarter 16 | 98.2 | 98.8 | 98.5 | 98.2 | 98.3 | 98.2 |
| Number of Programs Attended | 94.7 | 96.3 | 95.4 | 91.4 | 95.6 | 93.1 |
| Percentage of Weeks in Programs | 83.7 | 87.9 | 85.6 | 86.3 | 90.1 | 87.8 |

TABLE B.1 *(continued)*

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Males | Females | Total | Males | Females | Total |
| Hours per Week in Programs | | | | | | |
| All months | 82.7 | 87.2 | 84.8 | 85.8 | 89.7 | 87.3 |
| Quarter 1 | 90.8 | 94.1 | 92.3 | 94.4 | 95.9 | 95.0 |
| Quarter 5 | 93.9 | 95.6 | 94.7 | 96.1 | 96.9 | 96.4 |
| Quarter 16 | 97.8 | 98.4 | 98.1 | 97.7 | 97.9 | 97.8 |
| Attended Programs Other than Job Corps, by Type | | | | | | |
| Any | 96.8 | 98.3 | 97.5 | 96.5 | 98.4 | 97.2 |
| High school[c] | 94.0 | 95.3 | 94.6 | 93.6 | 95.8 | 94.4 |
| ABE or ESL[c] | 94.1 | 95.2 | 94.6 | 93.4 | 95.4 | 94.1 |
| GED[c] | 94.6 | 96.1 | 95.2 | 94.0 | 96.1 | 94.7 |
| Vocational/technical school | 95.2 | 96.7 | 95.8 | 94.7 | 97.3 | 95.7 |
| Two-year college | 95.0 | 96.5 | 95.7 | 94.3 | 96.9 | 95.3 |
| Four-year college | 94.7 | 96.3 | 95.5 | 94.2 | 96.6 | 95.1 |
| Percentage of Weeks in Programs Other than Job Corps | 87.3 | 90.4 | 88.7 | 85.8 | 89.7 | 87.3 |
| Hours per Week in Programs Other than Job Corps, by Type | | | | | | |
| Any | 87.3 | 90.4 | 88.7 | 85.8 | 89.7 | 87.3 |
| High school | 92.0 | 93.7 | 92.7 | 89.9 | 93.8 | 91.2 |
| GED | 92.0 | 93.7 | 92.7 | 90.8 | 92.5 | 91.4 |
| Vocational/technical school | 93.6 | 95.4 | 94.4 | 92.9 | 96.1 | 94.1 |
| Two-year college | 95.1 | 96.4 | 95.7 | 94.2 | 96.3 | 95.0 |
| Took Academic Classes | 48.5 | 45.6 | 47.2 | 48.0 | 46.6 | 47.4 |
| Weeks in Academic Classes | 39.2 | 37.8 | 38.5 | 42.2 | 41.9 | 42.1 |
| Hours per Week in Academic Classes | 39.2 | 37.9 | 38.6 | 42.2 | 42.0 | 42.2 |
| Took Vocational Training | 49.7 | 46.4 | 48.2 | 50.1 | 47.8 | 49.2 |
| Percentage of Weeks in Vocational Training | 42.5 | 40.8 | 41.7 | 46.9 | 45.4 | 46.4 |
| Hours per Week in Vocational Training | 42.7 | 40.8 | 41.9 | 46.9 | 45.4 | 46.4 |
| Degrees, Diplomas, and Certificates Received | | | | | | |
| GED certificate[b] | 98.8 | 99.1 | 98.9 | 99.1 | 99.0 | 99.0 |
| High school diploma[b] | 98.4 | 99.1 | 98.7 | 98.9 | 98.6 | 98.8 |
| Vocational/technical certificate | 99.3 | 99.4 | 99.3 | 99.2 | 99.1 | 99.2 |
| College degree (two-year or four-year) | 99.4 | 99.6 | 99.5 | 99.6 | 99.4 | 99.6 |
| Highest Grade Completed at 48 Months | 99.7 | 99.8 | 99.8 | 99.7 | 99.8 | 99.7 |
| **Employment and Earnings** | | | | | | |
| Employed, by Period | | | | | | |
| Quarter 1 | 94.3 | 96.3 | 95.2 | 94.4 | 97.2 | 95.5 |
| Quarter 5 | 97.5 | 98.5 | 97.9 | 97.6 | 98.3 | 97.8 |
| Quarter 16 | 98.1 | 98.4 | 98.3 | 98.2 | 98.7 | 98.4 |
| Year 1 | 95.9 | 97.5 | 96.6 | 95.7 | 98.0 | 96.6 |
| Year 2 | 98.2 | 98.6 | 98.4 | 98.0 | 98.8 | 98.3 |
| Year 3 | 98.6 | 98.6 | 98.6 | 98.3 | 98.4 | 98.4 |

TABLE B.1 *(continued)*

| Outcome Measure | Program Group |  |  | Control Group |  |  |
|---|---|---|---|---|---|---|
|  | Males | Females | Total | Males | Females | Total |
| Year 4 | 98.4 | 98.7 | 98.5 | 98.4 | 98.6 | 98.5 |
| Ever during the 48 months | 99.1 | 99.2 | 99.2 | 99.1 | 99.1 | 99.1 |
| Number of Jobs | 96.8 | 98.1 | 97.3 | 96.5 | 98.3 | 97.2 |
| Percentage of Weeks Employed, by Period |  |  |  |  |  |  |
| Quarter 1 | 91.8 | 94.7 | 93.1 | 92.9 | 95.5 | 93.8 |
| Quarter 5 | 95.3 | 96.6 | 95.9 | 95.6 | 96.6 | 96.0 |
| Quarter 16 | 97.0 | 97.7 | 97.3 | 96.9 | 97.4 | 97.1 |
| Year 1 | 90.9 | 94.0 | 92.3 | 91.4 | 94.7 | 92.6 |
| Year 2 | 93.1 | 95.2 | 94.1 | 93.3 | 95.2 | 94.0 |
| Year 3 | 92.6 | 94.6 | 93.5 | 92.2 | 94.5 | 93.1 |
| Year 4 | 94.4 | 96.1 | 95.2 | 94.3 | 95.5 | 94.7 |
| All months | 81.6 | 87.8 | 84.4 | 82.0 | 87.4 | 84.1 |
| Hours per Week Employed, by Period |  |  |  |  |  |  |
| Quarter 1 | 91.2 | 94.4 | 92.6 | 91.9 | 94.9 | 93.0 |
| Quarter 5 | 94.0 | 95.6 | 94.7 | 94.2 | 95.9 | 94.8 |
| Quarter 16 | 94.7 | 96.3 | 95.4 | 95.0 | 96.2 | 95.5 |
| Year 1 | 90.4 | 93.7 | 91.9 | 90.6 | 94.1 | 91.9 |
| Year 2 | 91.7 | 94.2 | 92.8 | 91.6 | 94.3 | 92.6 |
| Year 3 | 90.4 | 93.2 | 91.7 | 90.0 | 93.3 | 91.3 |
| Year 4 | 92.4 | 94.9 | 93.6 | 92.6 | 94.8 | 93.4 |
| All months | 79.9 | 86.4 | 82.8 | 80.5 | 86.3 | 82.7 |
| Earnings per Week, by Period |  |  |  |  |  |  |
| Quarter 1 | 91.2 | 94.4 | 92.6 | 91.9 | 94.9 | 93.0 |
| Quarter 5 | 94.0 | 95.6 | 94.7 | 94.2 | 95.9 | 94.8 |
| Quarter 16 | 94.6 | 96.3 | 95.4 | 95.0 | 96.1 | 95.4 |
| Year 1 | 90.4 | 93.7 | 91.9 | 90.6 | 94.1 | 91.9 |
| Year 2 | 91.7 | 94.2 | 92.8 | 91.6 | 94.3 | 92.6 |
| Year 3 | 90.4 | 93.2 | 91.7 | 90.0 | 93.3 | 91.3 |
| Year 4 | 92.3 | 94.8 | 93.5 | 92.5 | 94.7 | 93.3 |
| All months | 79.9 | 86.4 | 82.8 | 80.5 | 86.3 | 82.7 |
| Characteristics of the Most Recent Job in Quarter 16 for Those Employed |  |  |  |  |  |  |
| Number of months on job | 98.8 | 99.1 | 98.9 | 98.9 | 99.2 | 99.0 |
| Usual hours worked per week | 99.6 | 99.7 | 99.7 | 99.7 | 99.5 | 99.7 |
| Hourly wage | 99.6 | 99.7 | 99.7 | 99.7 | 99.5 | 99.7 |
| Weekly earnings | 99.6 | 99.7 | 99.7 | 99.7 | 99.5 | 99.7 |
| Occupation | 99.3 | 99.5 | 99.4 | 99.2 | 99.3 | 99.3 |
| Type of employer | 96.0 | 95.8 | 95.9 | 96.1 | 93.6 | 95.2 |
| Fringe benefits available |  |  |  |  |  |  |
| Health insurance | 98.1 | 98.5 | 98.3 | 97.1 | 98.4 | 97.6 |
| Paid sick leave | 97.3 | 97.9 | 97.5 | 97.0 | 96.8 | 96.9 |
| Paid vacation | 98.2 | 98.4 | 98.3 | 97.9 | 97.6 | 97.8 |
| Retirement or pension benefits | 94.9 | 95.1 | 95.0 | 95.3 | 93.4 | 94.6 |
| Employed or in an Education or Training Program, by Period |  |  |  |  |  |  |
| Quarter 1 | 94.6 | 96.8 | 95.6 | 93.3 | 96.2 | 94.4 |
| Quarter 5 | 97.2 | 98.5 | 97.8 | 97.5 | 98.2 | 97.7 |

TABLE B.1 *(continued)*

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Males | Females | Total | Males | Females | Total |
| Quarter 16 | 98.1 | 98.4 | 98.2 | 98.0 | 98.2 | 98.1 |
| Year 1 | 96.3 | 97.9 | 97.0 | 95.6 | 97.9 | 96.5 |
| Year 2 | 98.3 | 98.9 | 98.6 | 98.2 | 98.8 | 98.4 |
| Year 3 | 98.4 | 98.8 | 98.6 | 98.5 | 98.3 | 98.4 |
| Year 4 | 98.6 | 98.7 | 98.6 | 98.3 | 98.4 | 98.4 |
| Ever during the 48 months | 99.7 | 99.7 | 99.7 | 99.3 | 99.6 | 99.4 |
| Percentage of Weeks in Any Activity | 75.2 | 82.3 | 78.4 | 76.8 | 82.6 | 79.0 |
| Hours per Week in Any Activity | | | | | | |
| Quarter 1 | 85.1 | 89.8 | 87.2 | 87.5 | 91.6 | 89.1 |
| Quarter 5 | 88.8 | 91.9 | 90.2 | 90.8 | 93.3 | 91.8 |
| Quarter 16 | 93.1 | 94.9 | 93.9 | 93.0 | 94.3 | 93.5 |
| Year 1 | 84.1 | 89.0 | 86.3 | 86.0 | 90.5 | 87.7 |
| Year 2 | 85.4 | 89.7 | 87.3 | 88.0 | 91.1 | 89.2 |
| Year 3 | 85.9 | 89.1 | 87.3 | 86.3 | 89.6 | 87.5 |
| Year 4 | 90.1 | 92.5 | 91.2 | 89.7 | 92.1 | 90.6 |
| All months | 70.1 | 78.0 | 73.7 | 73.1 | 79.7 | 75.6 |
| **Receipt of Public Assistance** | | | | | | |
| Received AFDC/TANF, Food Stamps, SSI/SSA, or GA Benefits, by Period | | | | | | |
| All months | 93.4 | 97.7 | 95.3 | 93.2 | 97.8 | 94.9 |
| Year 1 | 94.0 | 97.1 | 95.4 | 93.4 | 97.1 | 94.8 |
| Year 2 | 95.2 | 97.8 | 96.4 | 94.3 | 97.8 | 95.7 |
| Year 3 | 96.4 | 97.7 | 97.0 | 96.6 | 97.9 | 97.1 |
| Year 4 | 97.6 | 98.1 | 97.8 | 97.9 | 98.2 | 98.0 |
| Number of Months Received Benefits | 88.3 | 91.5 | 89.7 | 87.6 | 90.8 | 88.8 |
| Amount of Benefits Received | 69.2 | 69.0 | 69.1 | 67.7 | 69.0 | 68.2 |
| Received AFDC/TANF Benefits, by Period | | | | | | |
| All months | 93.6 | 97.6 | 95.4 | 94.0 | 97.5 | 95.3 |
| Year 1 | 94.3 | 97.4 | 95.7 | 94.5 | 97.3 | 95.6 |
| Year 2 | 96.6 | 98.4 | 97.5 | 96.3 | 97.9 | 96.9 |
| Year 3 | 98.1 | 98.8 | 98.4 | 98.4 | 98.1 | 98.3 |
| Year 4 | 98.8 | 98.8 | 98.8 | 99.1 | 98.5 | 98.9 |
| Number of Months Received AFDC/TANF Benefits | 91.7 | 94.1 | 92.8 | 92.1 | 93.3 | 92.5 |
| Amount of AFDC/TANF Benefits Received | 81.1 | 81.7 | 81.4 | 81.3 | 82.2 | 81.6 |
| Received Food Stamp Benefits, by Period | | | | | | |
| All months | 95.6 | 98.1 | 96.7 | 95.5 | 97.9 | 96.4 |
| Year 1 | 96.2 | 97.9 | 97.0 | 95.8 | 98.0 | 96.7 |
| Year 2 | 98.2 | 99.0 | 98.6 | 97.7 | 98.9 | 98.1 |
| Year 3 | 98.2 | 98.6 | 98.4 | 98.3 | 98.6 | 98.5 |
| Year 4 | 98.9 | 98.7 | 98.8 | 98.8 | 98.5 | 98.7 |
| Number of Months Received Food Stamp Benefits | 93.5 | 94.4 | 93.9 | 92.5 | 93.9 | 93.1 |

TABLE B.1 *(continued)*

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Males | Females | Total | Males | Females | Total |
| Amount of Food Stamp Benefits Received | 77.5 | 78.2 | 77.8 | 76.7 | 78.7 | 77.4 |
| Received SSI/SSA Benefits | 95.0 | 97.4 | 96.1 | 95.3 | 97.9 | 96.3 |
| Number of Months Received SSI/SSA Benefits | 94.6 | 97.0 | 95.7 | 94.9 | 97.4 | 95.9 |
| Amount of SSI/SSA Benefits Received | 92.9 | 95.8 | 94.2 | 93.6 | 96.3 | 94.6 |
| Received GA Benefits | 93.6 | 95.7 | 94.5 | 93.6 | 95.9 | 94.5 |
| Number of Months Received GA Benefits | 93.5 | 95.3 | 94.3 | 93.1 | 95.6 | 94.0 |
| Amount of GA Benefits Received | 93.2 | 95.0 | 94.0 | 92.8 | 95.2 | 93.7 |
| Covered by Public Health Insurance | | | | | | |
| At 12 months | 93.1 | 97.6 | 95.1 | 93.3 | 97.4 | 94.9 |
| At 30 months | 95.0 | 99.0 | 96.8 | 95.7 | 98.1 | 96.6 |
| At 48 months | 96.3 | 99.1 | 97.6 | 96.2 | 98.9 | 97.2 |
| Received WIC Benefits (for females only) | NA | 98.5 | 98.5 | NA | 98.7 | 98.7 |
| Number of Months Received WIC Benefits (for females only) | NA | 95.7 | 95.7 | NA | 96.6 | 96.6 |
| Lived in Public Housing | | | | | | |
| At 12 months | 98.1 | 98.2 | 98.2 | 96.8 | 98.2 | 97.3 |
| At 30 months | 98.2 | 99.0 | 98.5 | 98.1 | 98.5 | 98.2 |
| At 48 months | 98.5 | 98.8 | 98.7 | 98.9 | 99.0 | 99.0 |
| Received UI Benefits | 96.4 | 98.2 | 97.2 | 96.6 | 98.5 | 97.3 |
| Number of Weeks Received UI Benefits | 96.1 | 97.9 | 96.9 | 96.0 | 97.9 | 96.7 |
| Amount of UI Benefits Received | 96.1 | 97.9 | 96.9 | 95.7 | 97.7 | 96.5 |
| Received Child Support | 99.5 | 99.5 | 99.5 | 99.6 | 99.5 | 99.6 |
| Amount of Child Support Received | 99.3 | 97.2 | 98.4 | 99.5 | 98.7 | 99.2 |
| Received Income from Friends | 99.1 | 99.4 | 99.2 | 99.5 | 99.4 | 99.5 |
| Amount of Income Received from Friends | 96.5 | 95.8 | 96.1 | 97.3 | 95.9 | 96.7 |
| Received Other Income | 99.1 | 99.4 | 99.2 | 99.4 | 99.2 | 99.3 |
| Amount of Other Income Received | 98.0 | 98.3 | 98.1 | 98.8 | 98.5 | 98.7 |
| **Involvement with the Criminal Justice System** | | | | | | |
| Arrested or Charged with a Delinquency or Criminal Complaint, by Period | | | | | | |
| Year 1 | 99.2 | 99.7 | 99.4 | 99.3 | 99.9 | 99.5 |
| Year 2 | 99.2 | 99.7 | 99.4 | 99.3 | 99.9 | 99.5 |
| Year 3 | 99.1 | 99.7 | 99.4 | 99.4 | 99.9 | 99.6 |
| Year 4 | 97.5 | 97.9 | 97.7 | 96.6 | 97.6 | 96.9 |
| All months | 99.8 | 99.9 | 99.8 | 99.7 | 99.9 | 99.8 |
| Number of Arrests | 98.6 | 99.5 | 99.0 | 98.9 | 99.9 | 99.2 |
| Months Until First Arrested | 97.5 | 98.0 | 97.7 | 97.3 | 97.8 | 97.5 |

TABLE B.1 *(continued)*

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Males | Females | Total | Males | Females | Total |
| Most Serious Charge for Which Arrested | 98.6 | 99.5 | 99.0 | 98.9 | 99.9 | 99.2 |
| Arrested for: | | | | | | |
| Murder | 98.6 | 99.5 | 99.0 | 98.9 | 99.9 | 99.2 |
| Assault | 98.7 | 99.6 | 99.1 | 99.0 | 99.9 | 99.3 |
| Robbery | 98.7 | 99.5 | 99.1 | 99.0 | 99.9 | 99.3 |
| Burglary | 98.7 | 99.5 | 99.1 | 99.0 | 99.9 | 99.4 |
| Larceny, vehicle theft, or other property crimes | 99.0 | 99.7 | 99.3 | 99.3 | 99.9 | 99.5 |
| Drug law violations | 98.9 | 99.6 | 99.2 | 99.3 | 99.9 | 99.5 |
| Other personal crimes | 98.9 | 99.6 | 99.2 | 99.0 | 99.9 | 99.4 |
| Other miscellaneous crimes | 99.4 | 99.7 | 99.6 | 99.4 | 99.9 | 99.6 |
| Convicted, Pled Guilty, or Adjudged Delinquent | 99.2 | 99.4 | 99.3 | 98.9 | 99.6 | 99.1 |
| Made a Deal or Plea-Bargained | 98.0 | 99.1 | 98.5 | 97.5 | 98.8 | 98.0 |
| Most Serious Charge for Which Convicted | 98.3 | 99.1 | 98.7 | 98.1 | 99.4 | 98.6 |
| Convicted of: | | | | | | |
| Murder | 97.2 | 98.9 | 98.0 | 97.0 | 99.2 | 97.8 |
| Assault | 97.3 | 98.9 | 98.1 | 97.1 | 99.2 | 97.9 |
| Robbery | 97.2 | 98.9 | 98.0 | 97.0 | 99.2 | 97.8 |
| Burglary | 97.4 | 98.9 | 98.1 | 97.1 | 99.2 | 97.9 |
| Larceny, vehicle theft, or other property crimes | 97.6 | 99.0 | 98.2 | 97.2 | 99.2 | 97.9 |
| Drug law violations | 97.3 | 98.9 | 98.0 | 97.3 | 99.2 | 98.0 |
| Other personal crimes | 97.3 | 99.0 | 98.1 | 97.1 | 99.2 | 97.9 |
| Other miscellaneous crimes | 98.1 | 99.0 | 98.5 | 97.7 | 99.4 | 98.3 |
| Served Time in Jail for Convictions | 99.2 | 99.4 | 99.3 | 98.9 | 99.6 | 99.1 |
| Weeks Spent in Jail | 97.5 | 99.2 | 98.3 | 96.6 | 99.5 | 97.7 |
| Put on Probation or Parole | 98.8 | 99.3 | 99.0 | 98.3 | 99.6 | 98.8 |
| **Tobacco, Alcohol, and Illegal Drug Use** | | | | | | |
| Smoked Cigarettes | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.8 | 99.8 | 99.9 | 99.8 |
| At 30 months | 99.7 | 100.0 | 99.8 | 100.0 | 99.8 | 99.9 |
| At 48 months | 99.7 | 99.8 | 99.7 | 99.9 | 99.8 | 99.8 |
| Consumed Alcoholic Beverages | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.9 | 99.9 |
| At 30 months | 99.7 | 100.0 | 99.8 | 99.9 | 99.8 | 99.9 |
| At 48 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.6 | 99.8 |
| Used Marijuana, Hashish, or Hard Drugs | | | | | | |
| At 12 months | 99.5 | 99.8 | 99.6 | 99.7 | 99.8 | 99.7 |
| At 30 months | 99.5 | 99.9 | 99.7 | 99.8 | 99.7 | 99.7 |
| At 48 months | 99.5 | 99.7 | 99.6 | 99.6 | 99.5 | 99.5 |
| Used Marijuana or Hashish | | | | | | |
| At 12 months | 99.6 | 99.8 | 99.7 | 99.7 | 99.9 | 99.8 |
| At 30 months | 99.6 | 99.9 | 99.8 | 99.9 | 99.8 | 99.9 |
| At 48 months | 99.6 | 99.7 | 99.6 | 99.7 | 99.6 | 99.7 |

TABLE B.1 *(continued)*

| Outcome Measure | Program Group Males | Program Group Females | Program Group Total | Control Group Males | Control Group Females | Control Group Total |
|---|---|---|---|---|---|---|
| Used Hard Drugs | | | | | | |
| At 12 months | 99.5 | 99.8 | 99.7 | 99.7 | 99.8 | 99.7 |
| At 30 months | 99.7 | 99.9 | 99.8 | 99.8 | 99.7 | 99.8 |
| At 48 months | 99.7 | 99.9 | 99.8 | 99.9 | 99.8 | 99.8 |
| Snorted Cocaine Powder | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.8 | 99.8 | 99.9 | 99.8 |
| At 30 months | 99.7 | 100.0 | 99.8 | 99.8 | 99.8 | 99.8 |
| At 48 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.6 | 99.7 |
| Smoked Crack Cocaine or Freebased | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.8 | 99.8 | 99.8 | 99.8 |
| At 30 months | 99.7 | 99.9 | 99.8 | 99.9 | 99.8 | 99.8 |
| At 48 months | 99.6 | 99.7 | 99.7 | 99.8 | 99.6 | 99.7 |
| Used Speed, Uppers, or Methamphetamines | | | | | | |
| At 12 months | 99.6 | 99.8 | 99.7 | 99.8 | 99.8 | 99.8 |
| At 30 months | 99.7 | 99.9 | 99.8 | 99.9 | 99.8 | 99.8 |
| At 48 months | 99.6 | 99.7 | 99.7 | 99.9 | 99.7 | 99.8 |
| Used Hallucinogenic Drugs | | | | | | |
| At 12 months | 99.6 | 99.8 | 99.7 | 99.8 | 99.9 | 99.9 |
| At 30 months | 99.7 | 99.9 | 99.8 | 99.9 | 99.8 | 99.8 |
| At 48 months | 99.6 | 99.7 | 99.6 | 99.9 | 99.7 | 99.8 |
| Used Heroin, Opium, Methadone, or Downers | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.9 | 99.9 |
| At 30 months | 99.7 | 99.9 | 99.8 | 99.9 | 99.8 | 99.9 |
| At 48 months | 99.6 | 99.7 | 99.7 | 99.8 | 99.5 | 99.7 |
| Used Other Drugs | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.9 | 99.8 |
| At 30 months | 99.6 | 99.9 | 99.8 | 99.9 | 99.7 | 99.8 |
| At 48 months | 99.6 | 99.7 | 99.7 | 99.8 | 99.7 | 99.8 |
| Shot or Injected Drugs with a Needle or Syringe | | | | | | |
| At 12 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.9 | 99.9 |
| At 30 months | 99.7 | 99.9 | 99.8 | 99.9 | 99.7 | 99.8 |
| At 48 months | 99.6 | 99.7 | 99.7 | 99.9 | 99.7 | 99.8 |
| In Alcohol or Drug Treatment | 99.6 | 99.9 | 99.8 | 99.8 | 99.9 | 99.8 |
| Weeks in Alcohol or Drug Treatment | 99.4 | 99.8 | 99.6 | 99.6 | 99.8 | 99.7 |
| **Health** | | | | | | |
| Health Status | | | | | | |
| At 12 months | 99.6 | 99.8 | 99.7 | 99.7 | 99.8 | 99.7 |
| At 30 months | 99.6 | 99.9 | 99.7 | 99.9 | 99.7 | 99.8 |
| At 48 months | 99.6 | 99.6 | 99.6 | 99.7 | 99.7 | 99.7 |

TABLE B.1 *(continued)*

| | Program Group | | | Control Group | | |
|---|---|---|---|---|---|---|
| Outcome Measure | Males | Females | Total | Males | Females | Total |
| Had Serious Physical or Emotional Problems That Limited the Amount of Work or Other Regular Activities That Could Be Done | | | | | | |
| At 12 months | 99.7 | 99.7 | 99.7 | 99.7 | 99.8 | 99.7 |
| At 30 months | 99.7 | 99.8 | 99.7 | 99.8 | 99.6 | 99.7 |
| At 48 months | 99.5 | 99.7 | 99.6 | 99.6 | 99.6 | 99.6 |
| **Fertility, Marriage, and Living Arrangements** | | | | | | |
| Had New Children | 99.3 | 99.5 | 99.4 | 99.5 | 99.4 | 99.4 |
| Number of New Children | 99.2 | 99.4 | 99.3 | 99.5 | 99.4 | 99.4 |
| Had Children out of Wedlock | 99.1 | 99.4 | 99.2 | 99.4 | 99.3 | 99.4 |
| Pregnant at 48 Months (for females) | NA | 99.3 | 99.3 | NA | 99.1 | 99.1 |
| Lived with All Children[d] | 97.2 | 98.9 | 98.2 | 97.6 | 98.9 | 98.2 |
| Time Spent with Noncustodial Children[e] | 94.7 | 82.6 | 92.5 | 95.7 | 87.8 | 94.4 |
| Provided Support for Noncustodial Children[e] | | | | | | |
| Any (such as food, toys, and money) | 92.7 | 80.9 | 90.5 | 93.7 | 84.7 | 92.3 |
| Money | 92.9 | 82.6 | 91.1 | 93.7 | 87.0 | 92.7 |
| Household Membership | 98.5 | 98.2 | 98.3 | 98.1 | 98.5 | 98.2 |
| Whether Youth Is the Household Head | 99.3 | 99.6 | 99.5 | 99.6 | 99.7 | 99.6 |
| Number in Household | 98.9 | 99.1 | 99.0 | 98.8 | 99.3 | 99.0 |
| Marital Status at 48 Months | 99.7 | 99.8 | 99.7 | 99.7 | 99.8 | 99.8 |
| **Child Care** | | | | | | |
| Ever Used Child Care | 98.4 | 99.2 | 98.8 | 98.5 | 99.4 | 98.8 |
| Ever Used Child Care by Relatives | 98.3 | 99.2 | 98.7 | 98.5 | 99.0 | 98.7 |
| Ever Used Child Care by Nonrelatives | 97.7 | 98.9 | 98.2 | 98.0 | 98.6 | 98.2 |
| Ever Used Day Care | 97.8 | 98.8 | 98.3 | 98.1 | 98.8 | 98.4 |
| Child Care Hours Per Week | 95.1 | 95.4 | 95.2 | 95.7 | 95.3 | 95.5 |
| Relative Child Care Hours per Week | 95.4 | 96.4 | 95.9 | 95.8 | 96.1 | 95.9 |
| Nonrelative Child Care Hours per Week | 97.6 | 98.4 | 98.0 | 98.0 | 97.9 | 98.0 |
| Day Care Hours per Week | 97.5 | 97.9 | 97.7 | 97.9 | 97.6 | 97.8 |
| **Mobility** | | | | | | |
| Distance in Miles Between Zip Codes of Residence at Application to Job Corps and at the 48-Month Interview | 96.6 | 98.3 | 97.3 | 96.1 | 97.2 | 96.5 |

TABLE B.1 *(continued)*

| Outcome Measure | Program Group Males | Program Group Females | Program Group Total | Control Group Males | Control Group Females | Control Group Total |
|---|---|---|---|---|---|---|
| Lived in Same State at Application to Job Corps and at the 48-Month Interview | 96.6 | 98.3 | 97.3 | 96.1 | 97.2 | 96.5 |
| **Sample Size** | **3,741** | **3,087** | **6,828** | **2,787** | **1,698** | **4,485** |

SOURCE: Baseline and 30-month, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: All figures are unweighted.

[a] Data pertain to program group members who enrolled in Job Corps.

[b] Data pertain to program group members who enrolled in Job Corps and had a 12- or 30-month interview.

[c] Data pertain to those without a high school credential at random assignment.

[d] Data pertain to those with children.

[e] Data pertain to parents who did not live with all their children.

NA = not applicable.

Missing values were somewhat more common for measures of *time* spent in key activities, because these measures were constructed using activity start and end dates, which sample members sometimes could not recall. Furthermore, data item nonresponse was more common for time measures covering longer periods than for those covering shorter periods. For example, the measures of quarterly hours employed were missing for about 5 percent of cases per quarter, whereas the measure of hours employed covering the entire 48-month period was missing for about 17 percent of cases.[11]

Measures of the amount of benefits that were received from the main public assistance programs (AFDC/TANF and food stamps) were missing for about 20 percent of all cases, primarily because some recipients did not remember or know the average monthly benefit amount that they received during a particular welfare spell.

Measures pertaining to academic and vocational training experiences were missing for more than one-half of sample members, for two reasons. First, there was a problem in the skip logic in the CATI program for the 30-month follow-up questionnaire. The error was corrected in April 1998, and thus the measures of academic and vocational training experiences are missing for about 55 percent of the 48-month sample who completed 30-month interviews before then. Consequently, the academic education and vocational training outcome measures were constructed only for those in the 48-month sample who (1) completed 30-month interviews after the error was corrected, and (2) did not complete 30-month interviews.[12]

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[11] Because of concerns about recall error, we set variables pertaining to the first year after random assignment to missing for all 253 cases who completed a baseline and 48-month interview but not a 12- or 30-month interview.

[12] The skip logic error affected program and control group members equally. Thus, the impact estimates on these outcomes are likely to be unbiased, although they may not be representative of all those in the study population.

Second, as discussed in the next section, the 48-month interview did not collect detailed information about enrollment in Job Corps. Thus, information on academic and vocational training experiences in Job Corps were missing for program group members who were enrolled in Job Corps during the period covered by the 48-month interview. Furthermore, these measures are missing for the small number of control group members who enrolled in Job Corps, because detailed survey information on Job Corps enrollment was never collected for these youths.

Data item nonresponse did not differ by research status or by gender.

## C. THE TREATMENT OF MISSING VALUES AND OUTLIERS

In this section, we discuss the treatment of missing values and outliers for key outcome measures used in the 48-month impact analysis. We begin with a detailed discussion of our approach for addressing these issues for the employment and earnings outcomes. We then provide a briefer description of similar procedures that were used for the other two categories of outcome measures.

### 1. Employment and Earnings

We constructed the key employment and earnings outcome measures using a weekly employment timeline for each youth. We used the timelines to determine the jobs held by sample members in each week during the 48-month (208-week) follow-up period, and used job start and end dates to construct them. Positive integers were used to signify that the youth was employed in a week, and a blank code signified that the youth was not working. If the reported *day* the job started or ended was missing, we set the day to "15." However, if the month or year was missing, then the relevant timeline entries were set to "missing" (using alphabetic codes). A timeline entry could have multiple codes. For example, a code of "1B" signified that the youth was working on the first job reported in the survey--job 1--in that week, but also that we were unsure whether the youth was working on job 2. A code of "13" signified that the youth was employed in jobs 1 and 3; a code of "AC" signified that we were unsure whether the youth was working on job 1 or on job 3, and so on.

Next, we describe our approach for constructing key employment-related outcome measures defined over specific periods: employment rates, weeks employed, hours employed, and earnings. We conclude with a brief discussion of the construction of variables describing the characteristics of the most recent job in quarter 10 and the most recent job in quarter 16.

### a. Employment Rates

Employment rates by quarter after random assignment were key outcome measures for the impact analysis. We calculated these rates using the employment timeline for each youth. For each quarter, we created an indicator variable that was set to "1" if the youth worked for at least 1 week during the quarter, "0" if the youth never worked and had no missing job codes, and to "missing" otherwise. The quarterly employment rates for the program and control groups were calculated as the weighted average of these employment indicator variables.

The missing values in the employment rate measures were due primarily to missing job start and end dates. We did not impute missing values for these outcomes. Thus, the raw employment rate measures were used in the impact analysis.

### b. Weeks Employed

The percentage of weeks employed in a quarter was also a key outcome measure for the impact analysis. We constructed this measure for each youth by dividing the number of weeks worked in the quarter by 13 (the number of weeks in a quarter). The number of weeks that a youth was employed was created by summing the weeks that the youth's employment timeline had positive codes. The variable was set to "0" if the youth was not employed each week, and it was set to "missing" if *any* timeline entry had a missing code but no positive code. For example, the variable was set to "missing" if a code was "A" but would *not* have been set to missing if a code was "1B," because the youth was known to have been working in job 1.

Importantly, nearly all missing values for the measures of weeks employed were for youth *who we knew worked,* but for whom we did not know for how long, because job start or end dates were missing. In contrast, variables for weeks worked were never missing for those who did not work, because they were set to "0" for these youths. Consequently, we were concerned that the mean value for the variables for the number of weeks worked were biased downwards (because the variables contain "too many zeroes" or "too few positive values") for *both* program and control group members. This problem could lead to biased impact estimates.

To address this concern, we used the following two steps to impute missing values for the time employed measures for those who we knew were employed:

1. We calculated the weighted mean number of weeks worked for those with positive values by gender, age, and race.
2. Workers with missing values were assigned the appropriate mean value according to their gender, age, and race.

The imputation procedure was performed *separately* for program and control group members.

This procedure is appealing, because the mean value of the adjusted weeks worked variable is equivalent to the product of (1) the proportion of those employed, and (2) the mean number of weeks worked for employed youths who originally had positive variable values. We refer below to this imputation procedure as the *zero-correction* imputation procedure.

It is noteworthy that we estimated impacts on the percentage of weeks employed by quarter using both the adjusted and unadjusted variables. As expected, the mean values for both the program and the control groups were higher using the adjusted measures, but the impact estimates were very similar. For example, in year 4 after random assignment, the average percentage of weeks employed using the adjusted measure was 60.2 percent for the program group and 57.2 percent for the control group (an impact of 3 percentage points). Using the unadjusted measure, the average percentage of weeks employed was 59.7 percent for the program group and 56.6 percent for the control group (an impact of 3.1 percentage points). We present the impact estimates using the adjusted measures in the impact report.

### c. Hours Employed per Week

To calculate measures of hours employed, we constructed for each youth an hours timeline that covered the 208-week follow-up period. A timeline entry signified the total number of hours that a youth worked in all jobs during the week. We created the hours timelines using the employment timelines and survey information on the number of hours per week that employed youths usually worked on their jobs. A timeline entry in a given week was set to "missing" if the employment timeline had a missing job code in that week. For example, we set the variable to "missing" if we found a code of "A" *or* "1B" (because we were unsure whether the youth worked in job 2 and, hence, whether to include hours worked in job 2). Total hours worked in a week was topcoded at 84 (12 hours worked per day for 7 days).

Using a regression approach, we imputed missing values for the variable on the number of hours per week that the youth usually worked on a job.[13] For those with positive values, we regressed usual hours worked on a set of control variables (that included demographic characteristics and other features of the job--the hourly wage, occupation, and available fringe benefits) using ordinary least squares (OLS) procedures.[14] Separate models were estimated for program and control group members. For missing cases, we computed predicted usual hours worked using the parameter estimates from the regression models. These predicted values were used in place of the missing values when we constructed the hours timelines.

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[13] The "usual hours" worked variable was missing for about 1 percent of jobs.

The "hours employed" outcome measures were obtained using the hours timelines. To calculate hours worked over a given period, we summed across entries in the hours timeline. The measures were set to "missing" if the hours timeline had any missing entries over the period.

We then adjusted the measures of hours employed using the zero-correction procedure to impute missing values for employed youths. We used these adjusted measures in the impact analysis.

### d. Earnings

We constructed the earnings measures using a weekly earnings timeline for each youth. A timeline entry was calculated by (1) multiplying, for each job the youth held during the week, the number of hours worked in the week and the hourly wage; and (2) summing these products over all jobs. The employment and hours timelines and hourly wage information were used to construct the earnings timelines. A timeline entry was set to "0" if the youth did not work in the week, and was set to "missing" if the relevant hours timeline entry was missing. However, a timeline entry was not set to "missing" if the hourly wage was missing, because missing hourly wages were imputed using the regression approach described above for imputing usual hours worked per week.[15,16]

We hand-checked cases that reported hourly wages less than $2.50 (about 2.5 percent of jobs) and greater than $15 (also about 2.5 percent of jobs). We looked at verbatim job descriptions and other job characteristics to determine whether outlier values were valid. About 90 percent of cases were determined to be valid.

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[14] The regression $R^2$ values were about .12.

[15] About 2 percent of jobs had missing wage information.

[16] The $R^2$ values from the wage regressions were about .22.

To check the robustness of study findings, we used several methods to treat hourly wages that we considered to be invalid. For example, (1) we imputed outliers using the regression model (which was our final approach), (2) we set outliers to missing, and (3) we set outliers less than $2.50 to $2.50 and outliers greater than $15 to $15. These procedures produced very similar impact estimates, because of the small number of outliers.

We calculated earnings over a given period by summing across entries in the earnings timeline, where each entry was converted into 1995 dollars with the GDP price deflator. Earnings were set to "0" for those who did not work during the period and to "missing" if any earnings timeline entry was missing during the period.

We then adjusted the earnings measures to impute missing values for workers using the zero-correction imputation procedure. In the 48-month impact report, we present estimated earnings impacts using the adjusted earnings measures. However, because earnings were the key outcome measure for the impact analysis, we estimated earnings impacts using various earnings constructs to test the sensitivity of study findings to alternative assumptions about how to treat missing values and outliers. As discussed, we constructed earnings measures using various assumptions about how to treat hourly-wage-rate outliers. In addition, we estimated impacts using adjusted earnings measures obtained using the zero-correction procedure and unadjusted measures. These procedures yielded very similar impact estimates. For example, the impact per eligible applicant on earnings per week in year 4 was $15.9 ($211.4 for the program group and $195.4 for the control group) using the adjusted earnings measure. The impact was $16.5 using the unadjusted earnings measure, and as expected, earnings levels were slightly smaller for both research groups ($208.7 for the program group and $192.2 for the control group).

### e. Characteristics of the Most Recent Job in Quarters 10 and 16

In the 48-month impact report, we present differences in the average characteristics of jobs held by program and control group members during quarters 10 and 16, including the hourly wage, job tenure, usual hours worked per week, weekly earnings, occupations, types of employers, and available fringe benefits. This analysis used information on the most recent job held by sample members during the 10th and 16th quarters after random assignment. We identified the most recent job in quarter 10 by searching for the most recent positive job code in the employment timeline between weeks 118 and 130, and identified the most recent job in quarter 16 by searching for the most recent positive job code in the employment timeline between weeks 196 and 208. For ties, we selected the job that the youth had held the longest.

The outcomes describing the characteristics of the most recent job in quarter 10 were conditional on having been employed in quarter 10, and similarly for the most recent job in quarter 16. Thus, we did not impute missing values, because we did not have the "zero" problem discussed above. We treated outliers in hourly wage rates using the same procedures described above, and converted hourly wages into 1995 dollars.

## 2. Education and Training

The procedures used to construct key education and training outcomes were very similar to those used to construct the employment-related outcomes. Using enrollment dates, we created weekly timelines that signified whether or not youths were enrolled in Job Corps or other education and training programs during each week of the follow-up period. These timelines were used to construct period-specific measures of participation in all education and training programs, participation in specific types of programs, and weeks spent in these programs.

Unlike the 12- and 30-month interviews, the 48-month interview did not contain a section about participation in Job Corps (because only a small number of sample members were enrolled in Job Corps during the period covered by the 48-month interview). Thus, we used Job Corps enrollment and termination dates from the Student Pay and Allotment Management Information System (SPAMIS) to extend the Job Corps timelines for the period covered by the 48-month interview.[17] SPAMIS data were used for about 5 percent of program group members who were enrolled in Job Corps between their previous interview and the 48-month interview. Only about 9 percent of all weeks spent in Job Corps were captured by these spells. SPAMIS data were also used to construct Job Corps timelines for control group members who ever enrolled in Job Corps, because none of the follow-up interviews collected direct information on Job Corps enrollment for these youths.

We also used the education and training timelines, along with information about usual hours per week spent in programs, to construct weekly hours timelines.[18] We used regression procedures to impute the small number of missing values for the variable on usual hours per week spent in programs.[19] Weekly hours in the timelines were topcoded at 48 hours. We constructed period-specific measures of hours spent in education and training programs using the hours timelines.

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[17] Some program group members reported that they attended Job Corps in the section of the 48-month follow-up interview on participation in education and training programs. However, Job Corps enrollment rates were substantially smaller using the 48-month survey data than SPAMIS data. This was *not* the case when program group members were *directly* asked about Job Corps participation during the 12- and 30-month follow-up interviews.

[18] We assumed that youths in Job Corps spent 40 hours per week in education and training.

Cases with missing values for the measures on time spent in education and training programs were primarily those who we know participated in programs but for whom program start and end dates were missing. Thus, we used the zero-correction procedure to impute missing values for these program participants. Separate imputation procedures were performed for different types of programs. These adjusted measures were used in the 48-month impact analysis.

We also created a weekly timeline that signified whether or not the youth was in academic classes during each week of the follow-up period, and another that signified whether or not the youth was in vocational training. We applied the procedures described above to these timelines to construct measures of time spent in academic classes and vocational training.[20] Because SPAMIS does not contain information on time spent in academic classes or vocational training, we used a regression procedure to impute the amount of instruction received in Job Corps during those periods in which SPAMIS data were used to construct the Job Corps timelines.

We did not impute missing values for outcomes pertaining to the receipt of degrees, diplomas, or certificates (for example, GED certificates, high school diplomas, vocational certificates, and college degrees). However, as discussed in the 48-month impact report, we constructed several measures of highest grade completed, because of inconsistencies in responses across interviews.

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[19] The control variables used in the regression models included demographic characteristics and other characteristics of the education or training program (such as the type of program and whether the youth took academic classes or vocational training). The regression $R^2$ values were about .13. About 1 percent of programs had missing values.

[20] The academic and vocational training hours timeline entries were each topcoded at 48 hours.

## 3. Nonlabor Market Outcomes

We constructed outcome measures on the receipt of public assistance benefits using very similar procedures to those used for the employment-related outcomes. We created monthly timelines on the receipt of various forms of public assistance benefits (AFDC/TANF, food stamps, GA, SSI/SSA, WIC, and UI) and used these timelines to construct measures of participation in these programs. For those who received benefits, we used the zero-correction procedure to impute missing values for the number of months that benefits were received.

To construct measures of the amount of benefits received, we used the welfare timelines and information on the monthly amount of benefits received for each spell of receipt. We used regression procedures to impute missing benefit amounts for AFDC/TANF and food stamp spells.[21] The control variables used in the models included gender, age, household composition, fertility history, region of residence, and employment and earnings measures.[22] We also identified outliers in usual monthly benefit amounts by hand-checking very large and very small values. We compared potential outliers with published statistics on monthly benefit amounts by household size, household composition, and state. We imputed outlier values using the regression models.

For the other nonlabor market outcomes, we did not adjust for missing values for any of the constructed *binary* (0/1) or categorical outcome measures. For example, we did not impute missing values for indicators of arrests, convictions, health status, marital status, or the presence of children. However, we used the zero-correction procedure to impute missing *continuous* variables that were conditional on other variables. For example, we imputed missing values for the time spent in jail for those who we know were incarcerated. Similarly, we imputed missing values for the time spent in drug or alcohol treatment for those who we know were treated.

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[21] The regression $R^2$ values were about .40 for the AFDC/TANF benefit amount models and about .20 for the food stamp benefit amount models. About 3 percent of AFDC/TANF spells and 3 percent of food stamp spells had missing benefit amounts.

[22] We imputed the small number of missing benefit amounts for SSI, GA, and UI spells using mean benefit amounts for program recipients with nonmissing values.

# APPENDIX C

# THE ADJUSTMENT FOR CROSSOVERS

## A. INTRODUCTION

About 1.4 percent of all control group members (and 1.2 percent of control group members in the 48-month sample) enrolled in Job Corps before their three-year restriction period ended. We refer to these youths as "early crossovers." In addition, 3.2 percent of control group members enrolled in Job Corps between three and four years after random assignment (that is, after their restriction period ended). We refer to these youths as "late crossovers." To preserve the integrity of the random assignment design, we treated crossovers as control group members in the analysis. Thus, impact estimates that do not account for these crossovers could be biased if crossovers benefited from participation in Job Corps.

The 48-month impact report describes in detail statistical procedures that we used to estimate impacts per eligible applicant and impacts per program participant that do not account for control group crossovers. We estimated impacts *per eligible applicant* by comparing the distribution of outcomes for *all* program and control group members. This procedure generates unbiased estimates, because random assignment was performed at the time applicants were determined to be eligible for Job Corps. We estimated impacts *per participant* that do not adjust for crossovers by dividing the impacts per eligible applicant by the proportion of program group members who enrolled in Job Corps (73 percent). These estimates are unbiased under the assumption that Job Corps has zero impact on eligible applicants who do not enroll in the program.

The impact report, however, only briefly discussed our approach for estimating impacts for crossovers. This appendix describes these procedures in more detail.

## B. THE ADJUSTMENT FOR EARLY CROSSOVERS

A small number of control group members enrolled in Job Corps before their three-year restriction period ended. As described in the report on study implementation (Burghardt et al. 1999), the Job Corps national office allowed most of these youths to remain at centers, but held outreach and admissions and center staff accountable for these errors. The average duration of stay in Job Corps for these youths (7.6 months) was very similar to the average duration of stay for program group enrollees (8 months). Thus, impact estimates on employment and earnings in the postprogram period that do not adjust for these crossovers could be slightly biased downwards if these crossovers benefited from participation in Job Corps.

The procedure to obtain impact estimates per participant in the absence of crossovers can be extended to accommodate early crossovers in the control group (Angrist et al. 1996). The modified procedure involves dividing the estimated impact per eligible applicant by the *difference* between the Job Corps enrollment rate (the "show" rate) for the program group (73 percent) and the crossover rate for the control group (1.2 percent).

To illustrate how this works, we divide the population of eligible applicants into four mutually exclusive groups. These groups are defined by whether each youth would or would not enroll in Job Corps if assigned to the program group, and by whether each youth would or would not enroll in Job Corps as an early crossover if assigned to the control group. The four groups are as follows:

1. **Never-takers.** These are youths who would not enroll in Job Corps if they were in the program group and would not enroll in Job Corps as an early crossover if they were in the control group.

2. **Compliers.** These are youths who would enroll in Job Corps if they were in the program group, but would not enroll in Job Corps if they were in the control group.

3. **Defiers.** These are youths who would not enroll if they were assigned to the program group, but would enroll if they were assigned to the control group.

4. **Always-takers.** These are youths who would enroll in Job Corps if they were in the program group and also would enroll in Job Corps if they were in the control group.

Because of random assignment, the study's observed program and control groups each include equal proportions of the four groups. Furthermore, we can decompose the impact per eligible applicant on an outcome measure into a weighted sum of the contrasts between program and control group members in each of the four groups above (that is, $I = p_N I_N + p_C I_C + p_D I_D + p_A I_A$, where $I$ is the impact per eligible applicant, $p_N$ is the proportion of never-takers in the study population, $I_N$ is the difference between the mean outcome of program and control group members in the never-taker group--the impact per never-taker--and similarly for compliers, defiers, and always-takers whose terms are subscripted by $C$, $D$, and $A$, respectively).

In this framework, controlling for early crossovers amounts to estimating the impact of Job Corps participation per complier.

The following two-by-two table shows whether never-takers, compliers, defiers, and always-takers would be enrollees or nonenrollees, based on their research status:

| | If Youth Were Assigned to the Program Group | |
|---|---|---|
| If Youth Were Assigned to the Control Group | Does Not Enroll | Enrolls |
| Does Not Enroll | Never-taker | Complier |
| Enrolls | Defier | Always-taker |

Importantly, *we do not know who in the study population is in which of the four groups,* because youths were assigned only to one research status. We do not know whether control group members who enrolled in Job Corps--the crossovers--were defiers or always-takers, because that would depend on whether they would have enrolled in Job Corps if they had instead been assigned to the program group. Furthermore, we do not know which program group members would have been crossovers if they had instead been assigned to the control group. Likewise, we do not know whether a program group member who enrolled in Job Corps was a complier or an always-taker.

As stated, we do not know which program and control group members are in which of the four groups. However, three identifying assumptions, each of which is plausible, enable us to estimate the impact per complier.

*First,* we assume that impacts per never-taker are zero. This is similar to the assumption we used to estimate impacts per participant in the absence of crossovers, that impacts on no-shows are zero.

*Second,* we assume that impacts per always-taker are zero. This assumption implies that the mean outcomes of always-takers in the program and control groups were identical because all these youths enrolled in Job Corps. In other words, the outcomes of always-takers would be the same if they enrolled as part of the program group or as part of the control group. This assumption is reasonable, because, as noted, the average duration of stay was similar for the early crossovers and program group enrollees, and both groups were enrolled in Job Corps at roughly the same time (soon after random assignment).

*Third,* we assume that there are no defiers. This is reasonable, because it is highly likely that a youth who would enroll as part of the control group would also enroll as part of the program group. In other words, no youths would enroll in Job Corps if they were told they could not enroll, but would not enroll if they were told they could enroll. As can be seen from the bottom row of the table, this assumption means that all control group crossovers were always-takers; that is, all early crossovers would have enrolled in Job Corps if they had been assigned to the program group.

Using these assumptions, we can write the impact per complier as the impact per eligible applicant divided by the proportion of compliers in the population (that is, $I_C = I/p_C$). Using the table above, the proportion of compliers in the population equals the show-rate minus the early crossover rate. This result follows from the fact that (1) the show rate equals the sum of the proportion of eligible applicants who were compliers and the proportion who were always-takers, and (2) the proportion who were always-takers equals the control group crossover rate because of the assumption that there were no defiers in the population.

Importantly, the impacts per complier were very similar to the impacts per program participant that do not adjust for the early crossovers, because the early crossover rate was very small. For example, we obtained the impacts per participant for the full sample by dividing the impact per eligible applicant by .73, whereas we obtained the impacts per complier by dividing the impact per eligible applicant by .718 (.73-.012).

Finally, in the impact report, we present the mean of each outcome measure for program group *compliers* (although for clarity, we refer to them as mean outcomes for program group *participants*). We cannot directly observe these mean outcomes, because we do not know which program group members were compliers. However, we can estimate them by noting that the mean value for an outcome measure for the *full* program group ($T$) can be written as a weighted average of the mean outcome for program group members in each of the four groups discussed above (that is, $T = p_N T_N + p_C T_C + p_D T_D + p_A T_A$). Under the assumption that there are no defiers (that is, $p_D = 0$), the mean for always-takers in the program group ($T_A$) equals the mean for the early crossovers in the control group (which is observed, and which we denote by $C_{CR}$), and the mean for never-takers in the program group ($T_N$) equals the mean for no-shows in the program group (which is also observed, and which we denote by $T_{NS}$). Thus, the mean outcome for program group compliers can be estimated using the following expression:

$$(1) \quad \bar{T}_C = \frac{\bar{T} - (1-p_S)\bar{T}_{NS} - p_{CR}\bar{C}_{CR}}{(p_S - p_{CR})},$$

where $p_S$ is the show rate for the program group and $p_{CR}$ is the control group early crossover rate.

## C. THE ADJUSTMENT FOR LATE CROSSOVERS

Control group members were allowed to enroll in Job Corps after their three-year restriction period ended. About 3.2 percent of control group members enrolled in the program between their third and fourth years after random assignment. The enrollment rate was 4.6 percent for those 16 and 17 at application to Job Corps, 2.7 percent for those 18 and 19, and 1.1 percent for those 20 to 24. About 55 percent of these late crossovers were enrolled in Job Corps during the last quarter of the four-year period.

The approach to accommodate the *early* crossovers cannot be used to accommodate the *late* crossovers. As discussed, the adjustment procedure for *early* crossovers assumes that the average outcomes of early crossovers in the control group were the same as the outcomes of those in the program group who would have been early crossovers had they instead been assigned to the control group (whom we label "would-be" early crossovers). This assumption (that impacts per always-taker are zero) is reasonable, because most early crossovers in the control group enrolled in Job Corps soon after random assignment and thus were in Job Corps at roughly the same time as the would-be early crossovers in the program group. Thus, average earnings during the postprogram period were probably similar for the two groups.

The *late* crossovers, however, enrolled in Job Corps more than three years after random assignment, whereas nearly all program group participants enrolled within one year. Thus, we cannot assume that the average outcomes of late crossovers in the control group were similar to those of would-be late crossovers in the program group. In other words, the assumption that impacts per always-taker are zero is not tenable in this context. Instead, average earnings late in the observation period were probably much lower for the late control group crossovers than for their program group counterparts, because more than half these control group members were enrolled in Job Corps during this period, and those who had left Job Corps had been out for only a short period. Consequently, impact estimates on postprogram employment and earnings that do not adjust for these late control group crossovers would probably be biased slightly upwards.

Our procedure to adjust for the late control group crossovers was to "assume" that these crossovers never enrolled in Job Corps, and to impute their employment and education outcomes covering the last five quarters of the 48-month period. We conducted the imputation procedure in two stages. In the first stage, we identified noncrossovers in the control group whose average demographic characteristics and employment and education experiences during the first two years after random assignment were similar to those of the late crossovers.[23] Second, we imputed the employment and education outcomes of late crossovers using the average outcomes of noncrossovers in the matched sample (by age and gender).[24]

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[23] We used propensity score procedures to select the matched sample. The probability that a control group member was a late crossover was regressed on a set of explanatory variables, and a predicted probability (propensity score) was calculated for each control group member. We then selected the matched sample of noncrossovers as those with the closest propensity scores to those of the crossovers.

[24] We did not impute other outcomes (such as crime and family formation measures) for the late crossovers.

# APPENDIX D

# THE CALCULATION OF SAMPLE WEIGHTS AND STANDARD ERRORS

## A. INTRODUCTION

This technical appendix describes the calculation of sample weights that were used in the 48-month impact analysis to obtain unbiased estimates of program impacts that could be generalized to the study population. Sample weights were needed to account for the sample and survey designs and for interview nonresponse. This appendix also discusses procedures for constructing standard errors of the impact estimates, which were used to conduct tests of the statistical significance of the impact estimates.

## B. CALCULATION OF SAMPLE WEIGHTS

For several reasons, youths in the study population had different probabilities of being included in the follow-up interview samples. First, youths had different probabilities of being assigned to the program and control groups, because sampling probabilities differed for various population subgroups. Second, as discussed in Appendix A, youths selected to the research sample had different probabilities of being included in the baseline interview sample, because (1) baseline interview attempts continued in the post-45-day period for sample members who lived in randomly selected areas only, and (2) youths in different types of areas (superdense, dense, and nondense) had different probabilities of being eligible for post-45-day baseline interviews. All youths in the selected in-person areas were eligible for follow-up interviews. However, only youths in the nonselected areas who completed baseline interviews within 45 days after random assignment were eligible for 12-, 30-, or 48-month follow-up interviews.

Next, we discuss how sample weights were constructed to account for these design features. We conclude the section with a discussion of our approach for adjusting the weights to account for the effects of nonresponse to the follow-up interviews.

### 1. Weights to Account for the Sample Design

Groups of youths in the study population had different probabilities of being selected to the research sample. Table D.1 displays selection probabilities by research status for youths in those subgroups for which sampling rates were constant. The sampling rates to the control group are displayed by gender and by whether the youth lived in one of the 57 areas sending the largest number of nonresidential students to Job Corps.[25] The sampling rates to the *program research* group are displayed by residential designation status obtained from the special study (ETA-652 Supplement) form. The control and program *research* group sampling rates are displayed also for youths who were sent for random assignment before and after August 16, 1995. This is because the probabilities that youths were assigned to the research sample were increased for likely nonresidential students at that time to compensate for the lower-than-expected flow of eligible applicants and the higher-than-expected program no-show rate during the first several months of sample intake.

The sampling probabilities displayed in Table D.1 were adjusted for the following sample members:

- Four youths in the program research group who were also randomly assigned to the program nonresearch group.[26] The selection probabilities for each of these youths is *2p*, where *p* is the relevant sampling probability from Table D.1 for each youth.

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[25] Sampling rates were higher in these 57 areas to meet sample size targets for nonresidential students.

[26] This occurred as the result of a small error in our random assignment program. Our computer program was designed to check whether each youth sent for random assignment had been previously randomly assigned and to randomly assign only new cases. However, our computer program did not check whether duplicate information on a youth was present *within* a batch of information sent to MPR for random assignment purposes. Once identified, this problem was corrected.

## TABLE D.1

## PROBABILITIES THAT ELIGIBLE APPLICANTS WERE SELECTED TO THE CONTROL AND PROGRAM RESEARCH GROUPS, BY SAMPLING STRATA

(Percentages)

| | Sampling Probability | |
|---|---|---|
| | Random Assignment Date Before 8/16/95 | Random Assignment Date on or After 8/16/95 |
| **Control Group** | | |
| Females in areas from which a low concentration of nonresidential Job Corps female students come | 5 | 5 |
| Females in 57 areas from which a high concentration of nonresidential Job Corps female students come | 8 | 9 |
| Males in areas from which a low concentration of nonresidential Job Corps female students come | 8 | 8 |
| Males in 57 areas from which a high concentration of nonresidential Job Corps female students come | 8 | 9 |
| **Program Research Group** | | |
| Residential designees | 10.7 | 11.1 |
| Nonresidential designees | 15.4 | 17.0 |
| **Number in Sample Universe** | **47,288** | **33,595** |

- Twenty-seven youths who were recruited by the Florida employment service office in Hialeah (FLESHI) and who were randomized to the research sample after March 27, 1995. A large proportion of youths recruited by FLESHI in early 1995 were assigned to the control group, and FLESHI staff expressed concern to Region 4 senior staff about the negative effects the evaluation was having on their reputation. To help smooth the flow of control group members who were recruited by FLESHI for the remainder of the sample intake period, all youths sent for random assignment after March 27, 1995, had the *same* probability of being assigned to the control group (and the same probability of being assigned to the program research group). Hence, all youths in a batch sent for random assignment were randomized together rather than in separate strata. The uniform sampling rates were set as the average of all the sampling probabilities of all FLESHI youths who were sent for random assignment prior to March 28, 1995. The sampling rates to the control group were set as follows: (1) 7.63 percent for those sent for random assignment between March 28, 1995, and August 15, 1995; and (2) 8.05 percent for those sent for random assignment after August 15, 1995. The sampling rates to the program research group were set as follows: (1) 11.62 percent for those sent for random assignment between March 28, 1995, and August 15, 1995; and (2) 12.04 percent for those sent for random assignment after August 15, 1995.

The sample design weight for a youth was constructed to be inversely proportional to the probability of selection to the research group to which the youth was selected.

### 2. Weights to Account for the Survey Design

In this section, we first discuss selection probabilities to the baseline interview sample. These probabilities are needed to construct the selection probabilities to the follow-up interview samples. Second, we discuss the selection probabilities to the 12-, 30-, and 48-month interview samples, and the construction of weights that account for both the sample and survey designs.

#### a. Selection Probabilities to the Baseline Interview Sample

As discussed in detail in Appendix A, baseline interviews were attempted by telephone with all youths in the research sample during the first 45 days after random assignment. However, only youths in randomly selected areas who were not reachable by telephone within the 45-day period were eligible for telephone or in-person interviews during the post-45-day period.[27] To select these areas, we divided the country into 16 superdense, 29 dense, and 75 nondense areas. We then selected all 16 superdense, 18 dense, and 29 nondense areas as those where youths would be eligible for post-45-day interviewing. To maximize the precision of the impact estimates, we selected different proportions of superdense, dense, and nondense areas for in-person interviewing, subject to the cost of conducting interviews in each type of area and the limitations of a fixed interview budget.

The within-45-day sample is a random sample of those in the study population reachable by telephone within 45 days. The post-45-day sample, however, is a *clustered* sample of those in the study population reachable by telephone after 45 days. Thus, the post-45-day sample is underrepresented in the baseline sample relative to their numbers in the study population, and those in superdense, dense, and nondense areas have different representations in the post-45-day sample.

We calculated the probability that a youth was selected to the baseline interview sample by multiplying the probability the youth was selected into the research sample (as described above) by a factor $f$, defined as follows:

$f = 1$ if the youth completed a baseline interview within the first 45 days after random assignment

$= 1$ if the youth lived in a superdense area at application to Job Corps

$= 1$ if the youth was in the control group and was designated for a nonresidential slot on the Supplemental ETA-652 form

$= 18/29$ if the youth completed a baseline interview between 45 and 270 days after random assignment and lived in a dense area at application to Job Corps
$= 29/75$ if the youth completed a baseline interview between 45 and 270 days after random assignment and lived in a nondense area at application to Job Corps

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[27] Control group members designated for nonresidential slots on the Supplemental ETA-652 form, however, were eligible for post-45-day interviews regardless of where they lived. This design feature was adopted to increase the precision of impact estimates for the small nonresidential program component.

The factor $f$ can be interpreted as the conditional probability that an eligible applicant was in the baseline sample given that the applicant was selected into the research sample.

### b. Selection Probabilities to the 12-, 30-, and 48-Month Follow-Up Interview Samples

As discussed, the following two groups of youths were eligible for 12-month interviews:

1. *All* youths in the randomly selected areas slated for in-person interviewing at baseline (whether or not they completed a baseline interview)
2. Youths not in the in-person areas at baseline who completed baseline interviews within 45 days after random assignment

Thus, selection probabilities to the 12-month interview sample were the *same* as selection probabilities to the baseline interview (ignoring the effects of interview nonresponse). The 300 youths in the in-person areas who completed the 12-month interview but not the full baseline interview were assigned the same selection probabilities to the 12-month sample as those who completed baseline interviews between 45 and 270 days after random assignment.

Selection probabilities to the 30-month interview sample were identical to the selection probabilities to the 12-month interview sample. The selection probabilities to the 48-month interview sample were also identical to those to the 12-month sample for control group members. However, for program group members, the 48-month selection probabilities were slightly smaller than the 12-month selection probabilities, because to reduce data collection costs, we randomly selected for 48-month interviewing 93 percent of program group members who were eligible for 48-month interviews.[28]

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[28] This subsampling, however, affected selection probabilities for all program group members *equally* because of random sampling.

The primary weights used in the 48-month impact analysis were adjusted for interview nonresponse (as discussed in the next section). However, to test the sensitivity of our estimates, we also conducted the analysis using unadjusted weights, which were constructed to be inversely proportional to the selection probabilities to the 48-month interview sample. For both the program and control groups, the weights were scaled to sum to the size of the study population--80,883 eligible applicants.

### 3. The Adjustment of Weights to Account for Nonresponse to the 48-Month Interview

The main analysis sample for the 48-month impact analysis included the 11,313 youths (6,828 program group and 4,485 control group members) who completed 48-month interviews. The effective response rate (that is, the response rate in the in-person areas) to the 48-month interview was 79.9 percent (81.5 percent for the program group and 77.8 percent for the control group). Because about one in five youths did not complete the interview, control group members in the analysis sample may not be fully representative of all control group members (respondents and nonrespondents), and the sample of program group members may not be fully representative of all program group members. If not corrected, the effects of interview nonresponse could lead to two problems:

1. ***The impact estimates could be biased.*** This would occur if the average baseline characteristics of control and program group respondents differed.
2. ***The impact estimates might not be generalizable to the study population.*** This would occur if the average characteristics of respondents and nonrespondents differed (regardless of whether or not the average characteristics of program group and control group respondents were similar).

In this section, we assess the effects of nonresponse to the 48-month interview on estimated impacts and discuss our approach for adjusting for these effects.[29]

### a. Assessing the Effects of Nonresponse

Our basic approach for assessing the effects of nonresponse was to compare the characteristics of respondents to the full sample of respondents and nonrespondents by using ETA-652 and ETA-652 Supplement data. These data were collected at program intake and thus were available for all interview respondents *and* nonrespondents. For the analysis, we selected data items that we believed were correlated with whether a youth was a respondent and with key study outcome measures. We did not use baseline interview data, because these data were not available for 48-month nonrespondents who did not complete the baseline interview.

We performed the analysis using *only* the 9,937 sample members who lived in the areas selected for in-person interviews at baseline. Youths in the nonselected areas were excluded from the analysis, because "nonrespondents" in these areas consisted of both those who would have and those who would not have completed baseline interviews in the post-45-day period if given the chance. Therefore, "true" nonrespondents can be identified only in the selected areas. This sample of nonrespondents, however, is representative of nonrespondents nationwide. The analysis sample contains 7,940 respondents to the 48-month interview (3,276 control group and 4,664 program group members) and 1,997 nonrespondents (936 control group and 1,061 program group members). We excluded from the analysis the 443 program group members in the in-person areas who were eligible for 48-month interviews but, in an effort to reduce data collection costs, were not released for interviewing.

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[29] We also adjusted for the effects of nonresponse to the 12- and 30-month interviews using the same procedure as described next for the 48-month sample (Schochet 2000). The sample of those who completed the 12- and 30-month interviews were used in the impact analysis to test the robustness of our findings using the 48-month sample.

We used standard statistical tests to assess the similarity of respondents and the full sample of respondents and nonrespondents in the in-person areas. We used univariate t-tests to compare variable means for binary and continuous variables and chi-squared tests to compare variable distributions for categorical variables.[30] In addition, we conducted a more formal multivariate analysis to test the hypothesis that key variable means and distributions are *jointly* similar. For this analysis, we estimated logit regression models where the probability a person was a respondent versus a nonrespondent was regressed on a set of youth characteristics. Chi-squared (log-likelihood) tests were used to assess whether the explanatory variables in the models were jointly statistically significant. We also conducted similar tests comparing the characteristics of *respondents* in the program and control groups.

There are some differences in the characteristics of respondents to the 48-month interview and the full sample of respondents and nonrespondents (Table D.2). For example, females and younger sample members were significantly more likely than their counterparts to complete an interview. In addition, response rates were significantly higher (1) for those in less populated areas than for those in more populated areas (such as PMSAs, MSAs, or superdense areas), (2) for those with children at program application than for those without children, (3) for those who had completed high school at program application than for those without a high school degree, (4) for those never convicted prior to application than for those convicted, and (5) for nonresidential designees than for residential designees. Furthermore, the explanatory variables in the logit models are jointly statistically significant at the 1 percent level of significance for both program and control group members.

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[30] The test statistics to test for differences between respondents and the full sample are the same as those to test for differences between respondents and nonrespondents only.

## TABLE D.2

## COMPARISON OF THE CHARACTERISTICS OF RESPONDENTS AND THE FULL SAMPLE OF RESPONDENTS AND NONRESPONDENTS TO THE 48-MONTH INTERVIEW, BY RESEARCH STATUS

(Percentages)

| Characteristic[a] | Control Group |  | Program Group |  |
|---|---|---|---|---|
|  | Respondents[b] | Respondents and Nonrespondents | Respondents[b] | Respondents and Nonrespondents |
| **Demographic Characteristics** |  |  |  |  |
| Male | 54.4*** | 57.7 | 55.8*** | 58.0 |
| Age at Application |  |  |  |  |
| 16 to 17 | 40.5 | 39.7 | 39.9 | 39.9 |
| 18 to 19 | 31.8 | 32.1 | 32.0 | 31.8 |
| 20 to 21 | 16.6 | 16.8 | 16.3 | 16.4 |
| 22 to 24 | 11.1 | 11.4 | 11.8 | 11.9 |
| (Average age) | 18.9** | 19.0 | 18.9 | 19.0 |
| Race/Ethnicity |  |  |  |  |
| White, non-Hispanic | 22.4** | 21.8 | 22.5* | 22.5 |
| Black, non-Hispanic | 51.5 | 51.2 | 52.8 | 52.1 |
| Hispanic | 18.6 | 19.5 | 17.8 | 18.3 |
| Other | 7.5 | 7.5 | 6.9 | 7.1 |
| Region |  |  |  |  |
| 1 | 5.5** | 5.5 | 5.3* | 5.5 |
| 2 | 8.1 | 9.2 | 8.5 | 8.8 |
| 3 | 14.1 | 14.3 | 13.9 | 14.0 |
| 4 | 22.3 | 21.4 | 22.9 | 22.4 |
| 5 | 9.6 | 9.7 | 9.8 | 9.9 |
| 6 | 13.5 | 13.4 | 14.0 | 13.9 |
| 7/8 | 12.0 | 11.2 | 12.3 | 11.9 |
| 9 | 9.9 | 10.4 | 8.9 | 9.0 |
| 10 | 5.1 | 5.0 | 4.4 | 4.7 |
| Size of City of Residence |  |  |  |  |
| Less than 2,500 | 5.9*** | 5.4 | 5.5** | 5.3 |
| 2,500 to 10,000 | 7.4 | 7.1 | 8.3 | 7.8 |
| 10,000 to 50,000 | 15.6 | 15.2 | 15.9 | 16.0 |
| 50,000 to 250,000 | 17.9 | 18.2 | 18.0 | 18.2 |
| 250,000 or more | 53.2 | 54.2 | 52.2 | 52.6 |
| PMSA or MSA Residence Status* |  |  |  |  |
| In PMSA | 41.5*** | 44.1 | 44.2*** | 45.2 |
| In MSA | 44.3 | 43.0 | 42.1 | 41.6 |
| In neither | 14.1 | 12.8 | 13.6 | 13.2 |

TABLE D.2 *(continued)*

| Characteristic[a] | Control Group |  | Program Group |  |
|---|---|---|---|---|
|  | Respondents[b] | Respondents and Nonrespondents | Respondents[b] | Respondents and Nonrespondents |
| Density of Area of Residence* |  |  |  |  |
| Superdense | 48.0*** | 49.6 | 51.0** | 51.4 |
| Dense | 27.1 | 26.8 | 25.1 | 25.3 |
| Nondense | 24.9 | 23.6 | 24.0 | 23.3 |
| Lived in 57 Areas with a Large Concentration of Nonresidential Females** | 40.3 | 40.0 | 37.2 | 37.3 |
| Legal U.S. Resident* | 98.9 | 98.8 | 98.5 | 98.6 |
| Job Corps Application Date |  |  |  |  |
| 11/94 to 2/95 | 21.8** | 21.7 | 24.4*** | 24.3 |
| 3/95 to 6/95 | 31.0 | 30.0 | 29.1 | 28.5 |
| 7/95 to 9/95 | 28.0 | 28.3 | 27.5 | 27.3 |
| 10/95 to 12/95 | 19.2 | 20.0 | 19.0 | 19.9 |
| **Fertility and Family Status** |  |  |  |  |
| Had Dependents*** | 18.5*** | 17.0 | 15.6** | 15.1 |
| Family Status |  |  |  |  |
| Family head | 14.7*** | 14.3 | 14.2** | 13.9 |
| Family member | 62.2 | 61.1 | 61.3 | 60.9 |
| Unrelated person | 23.1 | 24.6 | 24.5 | 25.2 |
| Average Family Size | 3.2*** | 3.2 | 3.2 | 3.2 |
| **Education** |  |  |  |  |
| Completed the 12th Grade | 22.1 | 21.8 | 21.9*** | 21.1 |
| **Welfare Dependence** |  |  |  |  |
| Public Assistance Receipt |  |  |  |  |
| Received AFDC | 29.2*** | 28.1 | 28.7 | 28.3 |
| Received other assistance | 15.0 | 14.5 | 15.1 | 15.3 |
| Did not receive | 55.8 | 57.3 | 56.2 | 56.4 |
| **Health** |  |  |  |  |
| Had Any Health Conditions That Were Being Treated | 3.4 | 3.2 | 3.5** | 3.2 |

TABLE D.2 *(continued)*

| | Control Group | | Program Group | |
|---|---|---|---|---|
| Characteristic[a] | Respondents[b] | Respondents and Nonrespondents | Respondents[b] | Respondents and Nonrespondents |
| **Crime** | | | | |
| Arrested in Past Three Years | 11.1** | 11.7 | 11.3 | 11.4 |
| Ever Convicted or Adjudged Delinquent | 5.3** | 5.8 | 5.4 | 5.6 |
| **Completion Status to Previous Interviews** | | | | |
| Baseline Interview Completion Status | | | | |
| Completed within 45 days | 91.6*** | 88.3 | 91.1*** | 89.2 |
| Completed between 46 and 270 days | 5.7 | 5.9 | 6.2 | 6.5 |
| Did not complete | 2.8 | 5.8 | 2.8 | 4.3 |
| Completed the 12-Month Interview | 94.5*** | 88.5 | 94.5*** | 91.2 |
| Completed the 30-Month Interview | 88.5*** | 77.9 | 88.1*** | 80.4 |
| **Anticipated Program Enrollment Information** | | | | |
| Designated for a Nonresidential Slot*** | 20.6*** | 19.7 | 15.1 | 14.8 |
| Designated for a CCC[c] | 12.4 | 12.4 | 13.1 | 13.0 |
| Designated for a High- or Medium-High-Performing Center[c] | 45.7 | 46.3 | 46.8 | 47.0 |
| Designated for a Large or Medium-Large Center[c] | 36.3* | 37.0 | 37.2 | 37.5 |
| **Sample Size** | **3,276** | **4,212** | **4,664** | **5,725** |

SOURCE: 48-month follow-up interview, ETA-652 and ETA-652 Supplement data.

NOTES:

1. The figures are calculated for those sample members who were eligible for a baseline interview after 45 days after random assignment. These youths lived in randomly selected (in-person) areas at application to Job Corps.
2. All figures are calculated using sample weights to account for the sample and survey designs.

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3. The following cases in the in-person areas are excluded from the calculations: (1) 97 cases (43 control group and 54 program group members) who died between random assignment and the 48-month interview date, (2) 63 cases (31 control and 32 program) who were determined to have enrolled in Job Corps prior to random assignment, and (3) 443 randomly selected program group members who were eligible for 48-month interviews but who were not released for 48-month interviews to reduce data collection costs.

[a] Significance levels pertain to tests of differences between respondents in the program and control groups.

[b] Significance levels pertain to tests of differences between respondents and nonrespondents in the respective research group.

[c] Figures are obtained using data on OA counselor projections about the centers that youths were likely to attend.

*Difference is significant at the .10 level, two-tailed test.
**Difference is significant at the .05 level, two-tailed test.
***Difference is significant at the .01 level, two-tailed test.

The characteristics of program and control group respondents are more similar (Table D.2). Only 3 of the 25 univariate test statistics are statistically significant at the 5 percent level (which is slightly larger than the 1.25 that is expected by chance for 25 independent tests), and the joint test statistic from the multivariate model is statistically insignificant. Thus, although there are some differences in the average baseline characteristics of respondents and nonrespondents in each research group, it does not appear that there are large differences in the average baseline characteristics of program and control group respondents.

### c. The Adjustment of the Weights

Because of the differences between the characteristics of respondents and nonrespondents, we adjusted the 48-month weights to account for the effects of nonresponse. The weights were adjusted so that the weighted baseline characteristics of interview respondents were similar, on average, to those of the full population of respondents and nonrespondents. To be sure, there may have been unmeasured differences between respondents and nonrespondents for which we cannot control. Consequently, our procedure cannot account for the full effects of interview nonresponse. However, because of the large number of data items in the ETA-652 and ETA-652 Supplement forms, we believe that our procedure can account for some important differences between respondents and nonrespondents.[31]

To construct the adjusted weights, we estimated models where the probability that a youth in the in-person areas completed the 48-month interview was regressed on a set of control variables.

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[31] Sample selection statistical procedures could be used to account for both measured and unmeasured differences between respondents and nonrespondents. However, to implement these procedures effectively, we would have had to find at least one "instrumental" variable that is correlated with interview response status but uncorrelated with unobservable factors associated with the outcome measures. As is often the case, we were unable to find credible instrumental variables. Consequently, we did not correct for potential nonresponse bias using these sample selection procedures.

We estimated the models using logit maximum likelihood techniques and estimated separate models for program and control group members.

We used the following four steps to construct the adjusted weights:

1. ***A predicted probability (propensity score) was created for each respondent and nonrespondent using estimates from the "best" logit model.*** The best logit model included only control variables with predictive power in the regression models. The control variables for the model using program group members included 0/1 indicator variables signifying (1) gender; (2) race; (3) region; (4) whether the youth was a family member or family head; (5) whether the youth lived in a superdense, dense, or nondense area at application; (6) the size of city of residence; (7) high school completion status; (8) whether the youth ever had any serious illnesses or injuries; and (9) application date to Job Corps. The models using control group members included 0/1 indicator variables signifying (1) gender; (2) region; (3) whether the youth needed a bilingual program in Job Corps; (4) whether the youth lived in an PMSA, MSA, or neither; (5) the size of city of residence; (6) family size; (7) whether the youth was a family member or family head; (8) whether the youth was arrested in the three years prior to program application; and (9) application date to Job Corps.[32]

2. ***Youths were divided into six groups on the basis of the size of their predicted probabilities.*** The first group consisted of the 5 percent of youths with the largest predicted probabilities, and the second group consisted of the 15 percent of youths with the next-highest predicted probabilities. The other four groups were divided by quintiles of the predicted probability distribution. For example, the third group consisted of those whose predicted probabilities were between the 60th and 80th percentiles of the predicted probability distribution, and the fourth group consisted of those between the 40th and 60th percentiles, and so on. Cluster analytic techniques were used to determine these groupings.

3. ***The weighted 48-month interview response rate was calculated for each of the six propensity score groups.*** The response rates ranged from about .71 to .89 for the program group, and .58 to .90 for the control group. The variation in the response rates

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[32] We did not include indicator variables signifying completion status to the baseline interview in the final models, because the response rate to the 48-month interview was much higher for those who completed full baseline interviews than for those who did not (82 percent, compared to 61 percent). Thus, the coefficient estimates on the baseline completion variables were much larger than those of the other control variables. Consequently, the addition of the baseline completion variables would largely determine the nonresponse adjustments to the sample weights. We do not believe that the differences between respondents and nonrespondents can be captured primarily by whether a sample member completed the baseline interview within 45 days, after 45 days, or not at all. Thus, we did not include these variables in the final models.

suggests that the control variables had some predictive power in explaining whether or not a youth was an interview respondent.

4. ***The adjusted weight for a youth was then constructed to be proportional to the product of the unadjusted weight and the inverse of the response rate in that youth's propensity score group.*** The weights for both the control and program groups were scaled to sum to 80,883 (the size of the study population).[33]

Using these adjusted weights, we found no differences between the observable characteristics of respondents and the full sample of respondents and nonrespondents for both research groups (not shown). The adjusted weights were the primary weights used to construct all impact estimates presented in the 48-month impact report.

## C. CALCULATION OF STANDARD ERRORS

Standard errors of the impact estimates were used to test the statistical significance of program impacts. The construction of these standard errors is complicated, because they must account for design effects due to unequal weighting of the data and due to the clustered portion of sample caused by the random selection of areas for post-45-day interviewing at baseline.

In this three-part section, we discuss how we calculated standard errors for the impacts presented in the 48-month impact report. In the first section, we discuss the estimation of standard errors for impacts per eligible applicant (that is, for the difference between the weighted mean outcomes of program and control group members). Second, we discuss the estimation of standard errors for impacts per Job Corps participant that adjust for the control group crossovers. Finally, we discuss how we conducted chi-squared tests to test for differences in the distributions of categorical outcome measures across the program and control groups.

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[33] The 48-month sample contains youths who completed 48-month interviews but who were not in the in-person areas at baseline. These youths were not included in the sample used to estimate the logit models. However, we constructed weights for them by calculating predicted probabilities using the parameter estimates from the logit models, and assigned these youths to one of the six groups discussed above on the basis of the size of their predicted probabilities. Each of these youths was then assigned the response rate in the appropriate propensity score group (which was created using only those who lived in the in-person areas at baseline).

### 1. Standard Errors for Impacts per Eligible Applicant

The impact per eligible applicant on a binary or continuous outcome was calculated by comparing the weighted mean outcomes of program and control group members. To obtain an expression for the standard error of this impact estimate, it is instructive to first express the mean outcome of the program group (or the control group) as follows:

(1) $\bar{y} \square \grave{a}\bar{y}_1 \square (1 \square \grave{a})[\dot{e}_s \bar{y}_{2s} \square \dot{e}_d \bar{y}_{2d} \square \dot{e}_n \bar{y}_{2n}]$,

where:

$\bar{y}$ = the overall weighted mean of the variable

$\bar{y}_1$ = the weighted mean (using the sample design weights) of those in the 48-month sample who completed baseline interviews within 45 days after random assignment

$\bar{y}_{2s}, \bar{y}_{2d}, \bar{y}_{2n}$
= the weighted mean (using the sample design weights) of those in superdense, dense, and nondense areas, respectively, who (1) completed a baseline interview in the post-45-day period, or (2) did not complete a baseline interview, but completed a 12-month interview--"combo"cases. These two groups are labeled the "post-45-day" group.

$\dot{e}_s, \dot{e}_d, \dot{e}_n$
= the proportion of the post-45-day population in superdense, dense, and nondense areas, respectively

$\hat{o}$ = the proportion of all potential baseline interview completers who would have completed the baseline interview within 45 days after random assignment

In order to use equation (1), we assume that the weight, $\acute{a}$ is the proportion of baseline interview completers and combo cases in the in-person areas who completed the baseline interview within 45 days after random assignment (which is about 88 percent). This assumes that baseline interview nonrespondents (except for combo cases) were split *proportionally* between the within-45-day and post-45-day populations. As discussed in Schochet (1998a), this is a reasonable assumption, because the characteristics at program intake of baseline interview nonrespondents, within-45-day responders, and post-45-day responders were similar.

The *variance* of the difference between the mean outcome of program and control group members can be written using equation (1) as follows:

$$(2)\quad var(\bar{I}) \square \eth^2 var(\bar{I}_I) \square (1 \square \eth)^2 [\grave{e}_s^2 var(\bar{I}_{2s}) \square \grave{e}_d^2 var(\bar{I}_{2d}) \square \grave{e}_n^2 var(\bar{I}_{2n})],$$

where $\bar{I}$ represents the difference between the program and control group means, and where the other parameters and subscripts were defined above. The standard error of the impact estimate is the square root of the variance expression in equation (2).

Next, we discuss the estimation of each of the variance components in equation (2).

### a. Variance Estimate of the Impact for the Within-45-Day Sample

Because the two samples are independent, the variance of the impact estimate for the within-45-day sample is simply the sum of the variances of the program and control group means. Thus, the following equation can be applied separately to each of the two groups:

$$(3)\quad var(\bar{y}_I) \square (1 \square g) deffw_I \frac{\acute{o}_I^2}{n_I},$$

where:

$\acute{o}_I^2$ = variance of the outcome measure in the within-45-day population

$g$ = proportion of the population that is sampled (which is assumed in all analyses to be the average sampling rates to the research sample--7.4 percent for control group members and 11.6 percent for program group members)

$n_1^*$ = within-45-day sample size

$deffw_1$ = design effect due to unequal sample design weights ($w$) (which equals $n_1 \sum w^2/(\sum w)^2$, and that is due to the fact that various population subgroups had different probabilities of being selected to the research sample)

An unbiased estimate of the unknown $\sigma_1^2$ is calculated in the usual way, and this estimate is inserted in place of $\sigma_1^2$ in equation (3).

### b. Variance Estimate of the Impact for the Post-45-Day Sample in Superdense Areas

All 16 superdense areas were selected as in-person areas. Thus, the post-45-day sample in the superdense areas is a random (not clustered) sample. Thus, the same procedure as discussed for the within-45-day sample can be used to estimate the variance of the impact for the post-45-day sample in the superdense areas.

### c. Variance Estimate of the Impact for the Post-45-Day Sample in Dense and Nondense Areas

Program and control group members in the post-45-day sample in dense or nondense areas may not be independent, because these youths were selected from the *same* areas. For example, the average characteristics of program and control group members who lived in the same areas may be correlated, because they may have faced similar local economic conditions and because people with similar characteristics tend to cluster in the same geographic areas. Thus, the average outcome measures for the two groups in the same area may be correlated.

The variance of the post-45-day impact in dense or nondense areas can be written as follows:

$$(4)\quad var(\bar{I}_2) = \left[\sigma_{2w}^2\left[\frac{(1-g_c)}{n_{2c}a} + \frac{(1-g_p)}{n_{2p}a}\right] + \frac{(1-f)\sigma_{2b}^2}{a}\right] deff_{2w},$$

where the subscripts $c$ and $p$ refer to the control and program groups, $a$ is the number of dense (or nondense) areas selected for post-45-day baseline followup, $f$ is the fraction of all dense (nondense) areas selected for post-45-day baseline followup, $n_{2c}$ and $n_{2p}$ are post-45-day program and control group sample sizes per dense (nondense) area, $deff_{2w}$ is the design effect due to unequal weighting (see the definitions in equation (3) above), and where the subscripts denoting dense or nondense areas have been dropped for notational simplicity.

The term $\acute{o}_{2b}^{2}$ in equation (4) represents the variance of $\bar{I}$ across areas. In other words, it represents the *extent to which the impacts varied across areas.* The term captures both the between-area variance in the mean measure as well as the correlation of the group means within areas. The term $\acute{o}_{2w}^{2}$ represents the variance of the measure within areas.

An unbiased estimate of the variance expression in equation (4) is as follows:

$$(5)\quad v\hat{a}r(\bar{I}) \square \left[ (1 \square f)\frac{s_b^2}{a} \square s_w^2 [\frac{f(1 \square g_c)}{n_c a} \square \frac{f(1 \square g_p)}{n_p a}] \right] deff_{2w}$$

where $s_b^2$ is the sample variance of the impacts between areas, $s_w^2$ is the (average) sample variance of the measure across youths within areas, and other subscripts are omitted for notational simplicity.

Because of small sample sizes, it is problematic to estimate the sample variance terms in equation (5) using post-45-day sample members only. This is because the response rate to the baseline interview was extremely high within the first 45 days after random assignment (89 percent) and only an additional 9 percent of the research sample in the in-person areas completed baseline interviews in the post-45-day period or were combo cases. Hence, the post-45-day sample is small. The 48-month sample contains only 156 post-45-day sample members (92 program and 64 control group members) who lived in the 18 selected dense areas and 163 post-45-day sample members (92 program and 71 control groups members) who lived in the 29 selected nondense areas. Hence, there were very few sample members in most of the selected dense and nondense areas, and there were none in several areas. Thus, the between-area and within-area variance estimates in the dense and nondense areas (that is, $s_b^2$ and $s_w^2$ ) would be imprecise if the post-45-day sample were used in the calculations.

To address this problem, we calculated the variance terms in the dense (and nondense) areas using the following two steps:

1. We estimated $s_b^2$ and $s_w^2$ in dense (nondense) areas using both the *within-45-day* and the *post-45-day* samples who lived in the selected dense (nondense) areas.
2. Using the estimated variances in step (1), we calculated equation (5) using *post-45-day* sample sizes.

This procedure assumes that the between-area and within-area variance estimates are similar for the within-45-day and post-45-day populations. This assumption cannot be reliably tested, because of small post-45-day sample sizes. However, we believe that it is sufficiently accurate and that our procedure yields more reliable variance estimates than those that would be obtained using only the post-45-day samples in the calculations.

We can then calculate an estimate of the total variance of the impact estimate, that is, of the expression in equation (2), using the estimated variances for the within-45-day and post-45-day samples. We estimated design effects by dividing this total variance estimate by an unbiased estimate of the variance of a simple random sample of the same size.

The total design effect for most measures based on the full baseline interview sample was about 1.08. Nearly the entire design effect was due to unequal sample weights. For two main reasons, only a small portion of the total design effect was due to clustering of the post-45-day sample. First, the clustered portion of the sample in the dense and nondense areas was very small, because of high baseline interview response rates within 45 days after random assignment. Second, impact estimates did not vary substantially across dense and nondense areas.

### 2. Standard Errors for Impacts per Job Corps Participant

In the 48-month impact report, we present estimated impacts per eligible applicant, as well as per Job Corps participant that adjust for the control group crossovers. We obtained the impact per participant on an outcome measure by dividing the estimated impact per eligible applicant by the *difference* between the proportion of program group members who enrolled in Job Corps and the proportion of control group members who enrolled in Job Corps during their three-year restriction period.[34] In mathematical terms, the estimated impact per participant ($I_p$) can be expressed as follows:

$$(6)\quad I_p = \frac{I}{(S-C)},$$

where $I$ is the estimated impact per eligible applicant, $S$ is the Job Corps participation (show) rate among the program group, and $C$ is the early crossover rate among the control group.

The variance of $I_p$ must account for *both* the variance of $I$ and the variance of $(S\text{-}C)$, because both these values were estimated from the sample. We used standard ratio estimator techniques to estimate the variance of the estimated impact per participant. Using a Taylor series approximation, we can write the variance of $I_p$ as follows:

$$(7)\quad var(I_p) \approx var[I - I_{p0}(S-C)]/(S_0-C_0)^2,$$

---

[34] For clarity, we refer to these impacts as impacts per participant for the remainder of this section, although it is technically correct to refer to them as impacts per complier.

where $I_{P0}$ is the "true" but unknown impact on participants, $S_0$ is the true but unknown show rate, and $C_0$ is the true but unknown early crossover rate. Using the definition of the variance of the sum of two random variables, equation (7) yields the following expression:

$$(8)\quad var(I_p) \square \frac{var(I) \square I^2_{P0}[var(S) \square var(C)] \square 2I_{P0}[cov(I,S) \square cov(I,C)]}{(S_0 \square C_0)^2}.$$

Equation (8) can be computed using the following procedure:

1. Replace $I_{P0}$ by the estimated impact per participant, $I_P$, using equation (6).

2. Replace $S_0$ by the estimated show rate, $S$, and replace $C_0$ by the estimated early crossover rate, $C$.

3. Calculate $var(S)$ using program group members, $var(C)$ using control group members, and the techniques for obtaining a standard error of a variable mean, as discussed in Schochet (1998a).

4. Note that the covariance of $I$ and $S$, $cov(I,S) = cov(\bar{y}-\bar{z},S) = cov(\bar{y},S)$, where $\bar{y}$ is the mean outcome measure for program group members and $\bar{z}$ is the mean outcome measure for control group members. Ignoring design effects due to clustering, the covariance term, $cov(\bar{y},S)$, can be estimated using the program group as follows:

   $cov(\bar{y},S) = (1-g)\ \acute{o}_{y,S} \square w_i^2 / (\square w_i)^2$,

   where $w_i$ is the weight for the i^th^ program group member, $g$ is the proportion of the study population that was sampled to the program group, and where:

   $\acute{o}_{y,S} = \square w_i (y_i - \bar{y})(S_i - S) / \square w_i$.

   In this expression, $y_i$ is the outcome for the ith program group member, and $S_i$ is 1 if the youth enrolled in Job Corps and zero otherwise.

5. The covariance of $I$ and $C$, $cov(I,C)=cov(\bar{z},C)$, was estimated using control group members and the same procedure as described in step 4 for estimating $cov(I,S)$.

The calculated t-statistics to test the statistical significance of the impacts per eligible applicant and the impacts per participant were nearly identical for all outcome measures. Thus, we draw the same conclusions about statistical significance for both sets of impact estimates. The results are so similar because the estimation errors in the show and early crossover rates were very small as a result of the large sample sizes. Thus, the estimated show and crossover rates could almost be treated as constants.

### 3. Significance Tests for Impacts on the Distribution of Categorical Variables

Thus far, we have discussed the construction of standard errors for binary and continuous variables. However, in the 48-month impact report, we also presented impacts on categorical variables (for example, the type of living arrangement at the 48-month interview or categories of total earnings over the 48-month period). To assess the statistical significance of these impact estimates, we used a modified chi-squared statistic to test whether the distribution of the categorical variables differed across the program and control groups. This test statistic was constructed by dividing the usual chi-squared statistic (appropriately weighted) by the average design effect across each level of the categorical variable (Scott and Rao 1981). We calculated this average design effect in two steps. First, using the methods from the previous section, we calculated the design effect for comparing the difference between group proportions for *each level* of the categorical variable. Second, we took a weighted average of these design effects.

Formally, we used the following equations to construct the chi-squared statistic:

(9) $\chi^2_{SR} = \dfrac{\chi^2_w}{\bar{d}}$ ,

(10) $\chi^2_w = \sum_{i=1}^{2} \sum_{j=1}^{J} \dfrac{(n_i p_{ij} - n_i p_j)^2}{n_i p_j}$ ,

(11) $p_j = \dfrac{n_1 p_{1j} + n_2 p_{2j}}{n_1 + n_2}$.

$$(12)\quad \bar{d} = \frac{1}{(J-1)} \sum_{j=1}^{J} (1-p_j) d_j \ ,$$

where $p_{ij}$ is the proportion of youths in group $I$ who are in category $j$, $n_i$ is the number of youths in group $I$, $p_j$ is the proportion of the study population in category $j$, and $d_j$ is the design effect for category $j$ as described above. Under the null hypothesis of no difference between group distributions, the chi-squared statistic is distributed chi-squared with $(J-1)$ degrees of freedom.

The modified chi-squared test statistic is intuitive. The statistic decreases as the average design effect increases. Thus, the hypothesis of no difference between group proportions is rejected less often as the average design effect (that is, the average variance across the categories) increases.

# APPENDIX E

# THE ESTIMATION OF REGRESSION-ADJUSTED IMPACTS

## A. INTRODUCTION

Many impact analysts report regression-adjusted impact estimates when using a random assignment design to evaluate the effectiveness of an intervention. Simple differences in the mean outcomes of program (treatment) and control group members yield unbiased estimates of program impacts in these evaluations. However, estimating impacts from multivariate models that control for other factors that affect the outcome measures can increase the precision of the estimated program impacts and the power of significance tests. In addition, the models can adjust for any random residual differences in the observable baseline characteristics of program and control group members.

As discussed in Appendixes A and D, the sample and survey designs for the National Job Corps Study are complex. It is fairly straightforward under this design to estimate program impacts that can be generalized to the study population using the simple differences-in-means estimation approach. Furthermore, because the 48-month analysis sample is large (6,828 program group and 4,485 control group members), the impact estimates for the full sample and most key subgroups are relatively precise. However, it is much more difficult to obtain unbiased impact estimates using the regression approach, because of the large number of weighting cells (sampling strata). Thus, while the regression approach may increase the precision of the impact estimates relative to the simple differences-in-means approach, these efficiency gains may be offset by the difficulty in obtaining regression-adjusted impact estimates that are unbiased and that can be generalized to all eligible applicants in the study population.

This appendix compares impact estimates on key outcomes using the regression and differences-in-means approaches and discusses our reasons for presenting the differences-in-means estimates in the 48-month impact report. The appendix is in four sections. First, we discuss impact estimation issues that account for the study design. Second, we discuss the control variables that were included in the regression models. Third, we present impact estimates and their standard errors on key outcome measures using the two approaches. Finally, we present our conclusions.

### 1. Impact Estimation Issues

As discussed in Appendix D, youths had different probabilities of being included in the follow-up interview samples, for two reasons:

1. Selection probabilities to the program research and control groups differed for various population subgroups.
2. For the baseline interview, only youths in randomly selected areas who could not be interviewed by telephone within 45 days after random assignment were eligible for telephone or in-person interviews during the post-45-day period. Furthermore, youths in different areas (superdense, dense, and nondense) had different probabilities of being eligible for post-45-day interviewing. Follow-up interviews were not attempted for those in the nonselected areas who did not complete baseline interviews within 45 days after random assignment.

This design yields 48 weighting cells (that is, strata with unique program research and control group probabilities of being included in the follow-up interview samples).[35]

As discussed in Appendix D, it is straightforward to estimate unbiased program impacts using the differences-in-means approach, because sample weights can be used to account for the design features discussed above. The use of sample weights ensures that the weighted distributions of the outcomes of control group members are representative of the outcomes of those in the study population if they had been assigned to the control group, and similarly for the weighted outcomes of program group members. In the 48-month impact analysis, the weight for a youth was constructed to be inversely proportional to the probability that the youth was included in the 48-month follow-up interview sample. The weights were also adjusted for the effects of nonresponse to the follow-up interviews. The estimation of standard errors of the impact estimates accounted for design effects due to unequal weighting of the data and clustering of the post-45-day sample.

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[35] There are 16 cells based on the sample design, because sampling rates differed by gender, residential/nonresidential designation status, whether the case lived in one of the 57 heavily nonresidential areas, and time period. Within each of the 16 cells, there are 3 cells due to the survey design defined by (1) cases who completed baseline interviews within the 45-day period and cases in superdense areas who completed baseline interviews in the post-45-day period, (2) those in dense areas who completed baseline interviews in the post-45-day period, and (3) those in nondense areas who completed baseline interviews in the post-45-day period.

Obtaining regression-adjusted impact estimates that account for the study design is more complex. The usual regression model, where the outcome measures are regressed on a program status indicator variable (which is 1 for program group members and 0 for control group members) and other control variables, can yield biased estimates of program impacts (that is, biased coefficient estimates on the program status indicator variable) because the estimates may be "weighted" incorrectly. Furthermore, estimating weighted regressions using the sample weights described above does not solve the problem (DuMouchel and Duncan 1983). To obtain unbiased impact estimates, separate regression-adjusted estimates must be obtained in each of the 48 weighting cells (many of which contain only a small number of sample members), and the weighted average of these 48 separate estimates must be calculated.

Specifically, unbiased regression-adjusted impacts can be obtained using the following procedure:

1. Define the 48 cells with unique pairs of control and program research group weights and assign each sample member to their weighting cell.
2. Estimate regression-adjusted impacts and standard errors within each of the 48 cells.
3. Obtain the overall regression-adjusted impacts as a weighted average of the regression-adjusted impacts in each cell, where a cell weight is the proportion of the study population within that cell.

4. Use a similar procedure to obtain the overall standard errors of the impact estimates.

This procedure is straightforward if there are few cells. For example, if the sampling rates to the control and program research groups differed only by gender (and if there were no clustering of the post-45-day baseline interview sample), then there would be only two cells. Regression-adjusted impacts could then be obtained by estimating separate models for males and females, and by taking a weighted average of the regression-adjusted impacts for males and females.

In the Job Corps study design, however, there are 48 potential cells, and 45 of them contain at least one sample member. Furthermore, there are many cells with few sample members. Having small numbers of sample members in some weighting cells necessitates aggregating across weighting cells, which could introduce some bias if impacts differ across the cells.

We estimated regression-adjusted impacts using four cells defined by gender and residential/nonresidential designation status. This grouping captures the key features of the sample design, and the sample sizes in each cell were large enough to facilitate subgroup analyses.[36] In addition, the impacts on key outcomes across the other weighting strata did not appear to differ substantially.[37]

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[36] The 48-month sample contains 5,954 male residents (2,581 controls), 574 male nonresidents (206 controls), 3,283 female residents (1,172 controls), and 1,502 female nonresidents (526 controls). The population weights were .55, .04, .31, and .10, respectively.

[37] We estimated separate models for the four cells (that is, a fully interacted model), because the parameter estimates on the control variables differed somewhat across the four cells. The use of F-tests led to the rejection of the hypothesis that the parameter estimates across the four groups were similar for several models that we estimated using different outcome measures.

### 2. Selecting Control Variables

The following two main criteria were used to select the control variables that we included in the regression models:

1. ***The variables should be "baseline" measures that pertain to the period prior to random assignment.*** Thus, the control variables were constructed using data from the baseline interview, program intake (ETA-652) forms, and special study (Supplemental ETA-652) forms. Potential control variables were those discussed in the report describing the baseline characteristics of youths served by Job Corps (Schochet 1998b), and in the report containing methodological appendixes on sample implementation and baseline interviewing (Schochet 1998a). In general, the control variables were binary. For example, we constructed 0/1 indicator variables for several groups defined by age, race and ethnicity, and months worked in the year prior to random assignment.[38]

2. ***The variables should have predictive power in regression models for key outcomes.*** For simplicity, the same set of variables was used to estimate impacts for all outcome measures. Thus, we selected a core set of control variables that were statistically significant in most (but not necessarily all) models.

Stepwise regression and other exploratory data-analytic methods were used to select the control variables. These methods were used to select variables that had predictive power in regression models for the following 12 key outcome measures that span the range of outcomes examined in the impact analysis:

1. Average earnings in year 4 after random assignment
2. Total earnings during the 48-month period

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[38] If a control variable was missing for less than 5 percent of cases, we replaced the missing values with mean values for the nonmissing cases by age, gender, and race/ethnicity. If a control variable was missing for more than 5 percent of cases, we constructed a missing indicator variable which was set to 1 for missing cases and 0 for nonmissing cases. In this case, the missing values for the original variable were set to 0 if the data item was a binary variable, but they were set to the mean value for the nonmissing cases if the data item was continuous. These rules were applied separately to data items that referred to all sample members (for example, whether the case ever worked or had a high school diploma), and to those that referred only to certain sample members (for example, the number of arrests for those ever arrested and the number of jobs for those who worked in the prior year).

3. Proportion of weeks worked in year 4

4. Average hours employed per week in year 4

5. Whether employed in quarter 16

6. Whether a GED was obtained (for those without a high school credential at random assignment)

7. Average hours per week spent in education and training programs during the 48-month period

8. Average months received AFDC/TANF benefits during the 48-month period

9. Average months received food stamp benefits during the 48-month period

10. Whether ever arrested during the 48-month period

11. Whether ever in jail during the 48-month period

12. Whether ever had a child during the 48-month period

Ordinary least squares (OLS) methods were used to estimate models for the continuous outcome measures (for example, average earnings in year 4). To estimate models for binary dependent variables (for example, whether the youth was ever arrested or had a child), we used both OLS (linear probability) and logit maximum likelihood methods. These models produced very similar results; we present the OLS results.

Table E.1 displays the list of control variables that were selected. The categories of variables include demographic characteristics, fertility and living arrangements, education and training experiences, employment and earnings, public assistance receipt, arrest experience, drug use, and health.

## TABLE E.1

## CONTROL VARIABLES INCLUDED IN THE REGRESSION MODELS TO OBTAIN REGRESSION-ADJUSTED IMPACT ESTIMATES

**Demographic Characteristics**

Age at Application to Job Corps
- 16 to 17
- 18 to 19
- 20 to 24

Race/Ethnicity
- White non-Hispanic
- Black non-Hispanic
- Hispanic
- American Indian, Alaskan Native, Asian, or Pacific Islander

Job Corps Region of Residence
- 1
- 2
- 3
- 4
- 5
- 6
- 7/8
- 9
- 10

PMSA or MSA Residence Status
- In PMSA
- In MSA
- In neither

Lived in One of 57 Areas Sending a Large Number of Nonresidential Females to Job Corps

Job Corps Application Date
- 11/94 to 2/95
- 3/95 to 6/95
- 7/95 to 9/95
- 10/95 to 12/95

Completed the Baseline Interview More Than 45 Days After Random Assignment

TABLE E.1 *(continued)*

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**Fertility and Living Arrangements at the Baseline Interview**

Had Own Children

Lived with Spouse or Partner

**Education and Training Experiences Prior to Random Assignment**

Had High School Diploma (not GED)

Had GED Certificate

Months in Education or Training in the Past Year
- 0
- 1 to 6
- 6 to 12
- Missing months in school

**Employment and Earnings Prior to Random Assignment**

Ever Worked

Employed in the Past Year

Months Employed in the Past Year
- 0 to 3
- 3 to 9
- 9 to 12
- Missing months employed

Earnings in the Past Year (in Dollars)
- Less than 1,000
- 1,000 to 5,000
- 5,000 to 10,000
- 10,000 or more
- Missing earnings in the past year

Currently Employed

TABLE E.1 *(continued)*

| **Public Assistance Receipt Prior to Random Assignment** |
|---|
| Received AFDC in the Past Year and a Missing Indicator Variable |
| Received Food Stamps in the Past Year and a Missing Indicator Variable |
| Lived in Public Housing |
| Family Was on Welfare for Most of the Time When Youth Was Growing Up |
| **Arrest Experience, Drug Use, and Health Prior to Random Assignment** |
| Ever Arrested |
| Smoked Marijuana or Hashish in the Past Year |
| Used Hard Drugs in the Past Year |
| Ever in Drug Treatment |
| Had Physical or Emotional Problems That Limited the Amount of Work That Could Be Done |

SOURCE: Baseline interview and ETA-652 data.

NOTE: Separate regressions were estimated for the following four groups: (1) males designated for residential slots, (2) males designated for nonresidential slots, (3) females designated for residential slots, and (4) females designated for nonresidential slots. Thus, control variables signifying gender and residential/nonresidential designation status were not included in the models.

### 3. Estimation Results

The regression $R^2$ values for the continuous variables were about .10 for the year 4 employment and earnings measures, .20 for the total earnings measure, and .15 for the measure on time spent in education and training. The $R^2$ values for the welfare receipt measures were about .35 for females but only .10 for males. Thus, except for the welfare receipt measures for females, the control variables explained only a small portion of the variance of the outcome measures. These findings suggest that the regression-adjusted approach does not substantially increase the precision of the impact estimates relative to the differences-in-means approach.

Tables E.2 to E.9 display estimated impacts per eligible applicant for the 12 outcome measures using the differences-in-means and regression approaches for the total sample and for key youth subgroups. The table also displays estimated standard errors of the impact estimates, the percentage reduction in the standard errors from using the regression approach, and p-values from t-tests to gauge the statistical significance of the impacts. The results are displayed for the total sample and for the following key youth subgroups: (1) males and females; (2) age at application to Job Corps (16 and 17, 18 and 19, and 20 to 24); and (3) residential and nonresidential designees.

The impact estimates are very similar using the two approaches. In addition, the p-values to test the statistical significance of the impacts are very similar. The reductions in the standard errors using the regression approach are small except for the welfare measures. Consequently, the same policy conclusions can be drawn using the two approaches for the full sample and for key population subgroups (including the small subgroups such as nonresidential designees).

Despite the similarity of the results using the two approaches, it is noteworthy that the impact estimates using the two approaches generally vary *more* than the standard errors. For example, the impacts on the proportion of weeks worked in year 4 differ by about 5 percent, whereas the standard

## TABLE E.2

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR THE FULL SAMPLE

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 15.9 | 3.77 | 0.000*** | 16.0 | 3.64 | 0.000*** | 3.3 |
| Entire 48-month period | 2.0 | 2.17 | 0.346 | 1.7 | 1.96 | 0.376 | 9.5 |
| Average Percentage of Weeks Employed in Year 4 | 3.0 | 0.75 | 0.000*** | 2.8 | 0.74 | 0.000*** | 2.0 |
| Average Hours Employed Per Week in Year 4 | 1.4 | 0.39 | 0.000*** | 1.4 | 0.38 | 0.000*** | 2.1 |
| Percentage Employed in Quarter 16 | 2.4 | 0.89 | 0.007*** | 2.2 | 0.88 | 0.012** | 0.6 |
| Received a GED Certificate[a] | 15.0 | 1.04 | 0.000*** | 14.7 | 1.04 | 0.000*** | -0.2 |
| Average Hours per Week Ever in Education or Training | 3.5 | 0.11 | 0.000*** | 3.5 | 0.11 | 0.000*** | 0.8 |
| Average Number of Months Received AFDC/TANF Benefits | -0.4 | 0.21 | 0.068* | -0.4 | 0.16 | 0.027** | 22.2 |
| Average Number of Months Received Food Stamp Benefits | -0.5 | 0.23 | 0.026** | -0.5 | 0.18 | 0.003*** | 21.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -3.7 | 0.87 | 0.000*** | -3.3 | 0.82 | 0.000*** | 5.7 |
| Percentage Served Time in Jail for Convictions | -2.1 | 0.71 | 0.003*** | -1.7 | 0.68 | 0.011** | 4.0 |
| Percentage Had New Children | 1.2 | 0.93 | 0.184 | 0.9 | 0.93 | 0.327 | 0.0 |
| **Sample Size** | **11,313** | | | **11,313** | | | |

TABLE E.2 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated for the following four groups: (1) males designated for residential slots, (2) males designated for nonresidential slots, (3) females designated for residential slots, and (4) females designated for nonresidential slots. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of the four groups was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.3

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR MALES

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 17.7 | 5.18 | 0.001*** | 18.3 | 5.09 | 0.000*** | 1.7 |
| Entire 48-month period | 3.0 | 2.99 | 0.314 | 2.4 | 2.77 | 0.393 | 7.2 |
| Average Percentage of Weeks Employed in Year 4 | 2.6 | 0.96 | 0.007*** | 2.5 | 0.94 | 0.009*** | 1.5 |
| Average Hours Employed per Week in Year 4 | 1.2 | 0.52 | 0.022** | 1.2 | 0.51 | 0.019** | 1.1 |
| Percentage Employed in Quarter 16 | 1.9 | 1.12 | 0.087* | 1.6 | 1.11 | 0.159 | 1.0 |
| Received a GED Certificate[a] | 13.6 | 1.31 | 0.000*** | 13.4 | 1.32 | 0.000*** | -0.7 |
| Average Hours per Week Ever in Education or Training | 3.5 | 0.14 | 0.000*** | 3.5 | 0.14 | 0.000*** | 1.1 |
| Average Number of Months Received AFDC/TANF Benefits | -0.4 | 0.13 | 0.004*** | -0.3 | 0.13 | 0.009*** | 3.0 |
| Average Number of Months Received Food Stamp Benefits | -0.6 | 0.17 | 0.001*** | -0.6 | 0.16 | 0.000*** | 2.6 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -5.1 | 1.20 | 0.000*** | -4.5 | 1.17 | 0.000*** | 2.5 |
| Percentage Served Time in Jail for Convictions | -3.0 | 1.05 | 0.004*** | -2.6 | 1.04 | 0.013** | 1.1 |
| Percentage Had New Children | 0.3 | 1.14 | 0.771 | 0.1 | 1.16 | 0.946 | -1.8 |
| **Sample Size** | **6,528** | | | **6,528** | | | |

TABLE E.3 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated by residential designation status. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of the two groups was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.4

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR FEMALES

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 14.1 | 5.08 | 0.006*** | 12.8 | 5.03 | 0.011** | 1.0 |
| Entire 48-month period | 1.1 | 2.86 | 0.696 | 0.8 | 2.64 | 0.754 | 7.5 |
| Average Percentage of Weeks Employed in Year 4 | 3.7 | 1.20 | 0.002*** | 3.4 | 1.18 | 0.004*** | 1.8 |
| Average Hours Employed per Week in Year 4 | 1.8 | 0.57 | 0.002*** | 1.6 | 0.57 | 0.005*** | 0.6 |
| Percentage Employed in Quarter 16 | 3.1 | 1.43 | 0.029** | 3.2 | 1.43 | 0.028** | 0.0 |
| Received a GED Certificate[a] | 17.5 | 1.72 | 0.000*** | 16.7 | 1.70 | 0.000*** | 1.5 |
| Average Hours per Week Ever in Education or Training | 3.4 | 0.19 | 0.000*** | 3.5 | 0.19 | 0.000*** | 0.3 |
| Average Number of Months Received AFDC/TANF Benefits | -0.5 | 0.42 | 0.247 | -0.4 | 0.35 | 0.270 | 18.1 |
| Average Number of Months Received Food Stamp Benefits | -0.6 | 0.47 | 0.222 | -0.4 | 0.38 | 0.249 | 17.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -1.5 | 1.09 | 0.174 | -1.7 | 1.09 | 0.121 | 0.3 |
| Percentage Served Time in Jail for Convictions | -0.6 | 0.71 | 0.389 | -0.5 | 0.72 | 0.492 | -1.3 |
| Percentage Had New Children | 2.3 | 1.51 | 0.134 | 2.1 | 1.53 | 0.168 | -1.2 |
| **Sample Size** | **4,785** | | | **4,785** | | | |

TABLE E.4 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated by residential designation status. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of the two groups was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.5

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR 16- AND 17-YEAR-OLDS

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 13.3 | 5.57 | 0.017** | 12.5 | 5.43 | 0.021** | 2.5 |
| Entire 48-month period | 6.7 | 3.02 | 0.027** | 6.7 | 2.75 | 0.015** | 9.1 |
| Average Percentage of Weeks Employed in Year 4 | 2.7 | 1.14 | 0.020** | 2.3 | 1.15 | 0.045** | -0.6 |
| Average Hours Employed per Week in Year 4 | 1.2 | 0.60 | 0.042** | 1.1 | 0.59 | 0.069* | 0.8 |
| Percentage Employed in Quarter 16 | 1.9 | 1.41 | 0.188 | 2.1 | 1.43 | 0.151 | -1.4 |
| Received a GED Certificate[a] | 13.6 | 1.43 | 0.000*** | 13.3 | 1.45 | 0.000*** | -1.2 |
| Average Hours per Week Ever in Education or Training | 2.6 | 0.17 | 0.000*** | 2.7 | 0.18 | 0.000*** | -1.0 |
| Average Number of Months Received AFDC/TANF Benefits | -0.6 | 0.29 | 0.052* | -0.4 | 0.27 | 0.126 | 7.2 |
| Average Number of Months Received Food Stamp Benefits | -0.6 | 0.31 | 0.046** | -0.5 | 0.28 | 0.091* | 7.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -3.4 | 1.43 | 0.019** | -3.4 | 1.36 | 0.013** | 5.2 |
| Percentage Served Time in Jail for Convictions | -3.5 | 1.22 | 0.004*** | -3.0 | 1.15 | 0.009*** | 6.0 |
| Percentage Had New Children | 0.7 | 1.44 | 0.604 | 0.8 | 1.46 | 0.568 | -1.4 |
| **Sample Size** | **4,649** | | | **4,649** | | | |

TABLE E.5 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated for the following four groups: (1) males designated for residential slots, (2) males designated for nonresidential slots, (3) females designated for residential slots, and (4) females designated for nonresidential slots. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of the four groups was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.6

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR 18- AND 19-YEAR-OLDS

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 3.9 | 6.78 | 0.566 | 4.2 | 6.73 | 0.538 | 0.7 |
| Entire 48-month period | -9.2 | 3.82 | 0.015** | -10.0 | 3.61 | 0.006*** | 5.5 |
| Average Percentage of Weeks Employed in Year 4 | 1.5 | 1.35 | 0.253 | 1.6 | 1.35 | 0.229 | 0.0 |
| Average Hours Employed per Week in Year 4 | 0.2 | 0.70 | 0.755 | 0.4 | 0.70 | 0.572 | 0.3 |
| Percentage Employed in Quarter 16 | 1.4 | 1.57 | 0.358 | 0.6 | 1.58 | 0.698 | -0.8 |
| Received a GED Certificate[a] | 16.4 | 1.93 | 0.000*** | 16.4 | 1.98 | 0.000*** | -2.3 |
| Average Hours per Week Ever in Education or Training | 3.6 | 0.20 | 0.000*** | 3.7 | 0.20 | 0.000*** | -1.4 |
| Average Number of Months Received AFDC/TANF Benefits | -0.2 | 0.37 | 0.658 | -0.2 | 0.29 | 0.505 | 21.5 |
| Average Number of Months Received Food Stamp Benefits | 0.0 | 0.43 | 0.936 | -0.1 | 0.33 | 0.784 | 21.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -4.7 | 1.51 | 0.002*** | -4.6 | 1.48 | 0.002*** | 2.5 |
| Percentage Served Time in Jail for Convictions | -1.0 | 1.21 | 0.412 | -0.9 | 1.20 | 0.473 | 1.1 |
| Percentage Had New Children | 0.8 | 1.68 | 0.621 | 0.0 | 1.71 | 0.999 | -1.8 |
| **Sample Size** | **3,577** | | | **3,577** | | | |

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated for the following four groups: (1) males designated for residential slots, (2) males designated for nonresidential slots, (3) females designated for residential slots, and (4) females designated for nonresidential slots. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of the four groups was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.7

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR 20- TO 24-YEAR-OLDS

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 33.5 | 7.50 | 0.000*** | 33.6 | 7.77 | 0.000*** | -3.5 |
| Entire 48-month period | 7.3 | 4.55 | 0.110 | 6.4 | 4.47 | 0.154 | 1.8 |
| Average Percentage of Weeks Employed in Year 4 | 5.0 | 1.45 | 0.001*** | 4.6 | 1.51 | 0.002*** | -4.4 |
| Average Hours Employed per Week in Year 4 | 3.0 | 0.75 | 0.000*** | 2.7 | 0.78 | 0.000*** | -4.3 |
| Percentage Employed in Quarter 16 | 4.3 | 1.63 | 0.009*** | 3.5 | 1.72 | 0.040** | -5.1 |
| Received a GED Certificate[a] | 17.3 | 2.45 | 0.000*** | 17.9 | 2.72 | 0.000*** | -10.8 |
| Average Hours per Week Ever in Education or Training | 4.6 | 0.23 | 0.000*** | 4.7 | 0.24 | 0.000*** | -2.8 |
| Average Number of Months Received AFDC/TANF Benefits | -0.4 | 0.45 | 0.388 | -0.3 | 0.31 | 0.267 | 30.7 |
| Average Number of Months Received Food Stamp Benefits | -1.1 | 0.53 | 0.037** | -1.1 | 0.40 | 0.007*** | 24.4 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -3.0 | 1.46 | 0.039** | -1.5 | 1.52 | 0.336 | -4.1 |
| Percentage Served Time in Jail for Convictions | -1.2 | 1.13 | 0.267 | 0.0 | 1.18 | 0.968 | -5.3 |
| Percentage Had New Children | 2.6 | 1.74 | 0.141 | 2.8 | 1.86 | 0.133 | -6.7 |
| **Sample Size** | **3,087** | | | **3,087** | | | |

TABLE E.7 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated for the following four groups: (1) males designated for residential slots, (2) males designated for nonresidential slots, (3) females designated for residential slots, and (4) females designated for nonresidential slots. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of the four groups was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.8

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR RESIDENTIAL DESIGNEES

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 15.7 | 4.14 | 0.000*** | 15.6 | 3.99 | 0.000*** | 3.8 |
| Entire 48-month period | 1.9 | 2.39 | 0.437 | 1.5 | 2.15 | 0.484 | 9.8 |
| Average Percentage of Weeks Employed in Year 4 | 2.9 | 0.83 | 0.000*** | 2.7 | 0.81 | 0.001*** | 2.3 |
| Average Hours Employed per Week in Year 4 | 1.4 | 0.43 | 0.001*** | 1.3 | 0.42 | 0.001*** | 2.7 |
| Percentage Employed in Quarter 16 | 2.4 | 0.97 | 0.014** | 2.1 | 0.97 | 0.026** | 1.0 |
| Received a GED Certificate[a] | 15.2 | 1.13 | 0.000*** | 15.0 | 1.13 | 0.000*** | -0.3 |
| Average Hours per Week Ever in Education or Training | 3.5 | 0.12 | 0.000*** | 3.5 | 0.12 | 0.000*** | 0.9 |
| Average Number of Months Received AFDC/TANF Benefits | -0.4 | 0.19 | 0.060* | -0.3 | 0.17 | 0.066* | 13.4 |
| Average Number of Months Received Food Stamp Benefits | -0.6 | 0.22 | 0.013** | -0.5 | 0.19 | 0.006*** | 13.1 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -4.1 | 0.98 | 0.000*** | -3.8 | 0.92 | 0.000*** | 5.9 |
| Percentage Served Time in Jail for Convictions | -2.5 | 0.80 | 0.002*** | -2.1 | 0.77 | 0.007*** | 4.8 |
| Percentage Had New Children | 1.6 | 1.01 | 0.111 | 1.3 | 1.01 | 0.205 | -0.2 |
| **Sample Size** | **9,237** | | | **9,237** | | | |

TABLE E.8 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated by gender. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of gender group was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

## TABLE E.9

## IMPACTS ON KEY OUTCOMES USING THE DIFFERENCES-IN-MEANS AND REGRESSION APPROACHES, FOR NONRESIDENTIAL DESIGNEES

| | Differences-in-Means Approach | | | Regression Approach | | | |
|---|---|---|---|---|---|---|---|
| Outcome Measure | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Estimated Impact per Eligible Applicant | Standard Error | P-Value | Percentage Reduction in the Standard Error |
| Average Earnings per Week (in 1995 Dollars) | | | | | | | |
| Year 4 | 17.0 | 8.76 | 0.053* | 18.9 | 8.82 | 0.033** | -0.7 |
| Entire 48-month period | 3.1 | 5.05 | 0.542 | 3.2 | 4.70 | 0.499 | 6.9 |
| Average Percentage of Weeks Employed in Year 4 | 3.3 | 1.77 | 0.060* | 3.7 | 1.78 | 0.037** | -0.7 |
| Average Hours Employed per Week in Year 4 | 1.3 | 0.88 | 0.137 | 1.5 | 0.89 | 0.090* | -1.6 |
| Percentage Employed in Quarter 16 | 2.4 | 2.06 | 0.243 | 2.7 | 2.10 | 0.206 | -1.9 |
| Received a GED Certificate[a] | 14.1 | 2.57 | 0.000*** | 13.2 | 2.65 | 0.000*** | -3.0 |
| Average Hours per Week Ever in Education or Training | 3.4 | 0.28 | 0.000*** | 3.5 | 0.28 | 0.000*** | -1.0 |
| Average Number of Months Received AFDC/TANF Benefits | -0.4 | 0.72 | 0.594 | -0.7 | 0.54 | 0.208 | 25.1 |
| Average Number of Months Received Food Stamp Benefits | 0.0 | 0.77 | 0.974 | -0.6 | 0.56 | 0.294 | 26.9 |
| Percentage Arrested or Charged with a Delinquency or Criminal Complaint | -1.7 | 1.72 | 0.316 | -0.5 | 1.67 | 0.772 | 3.0 |
| Percentage Served Time in Jail for Convictions | 0.1 | 1.24 | 0.934 | 0.3 | 1.24 | 0.790 | 0.1 |
| Percentage Had New Children | -0.9 | 2.23 | 0.695 | -1.4 | 2.28 | 0.537 | -2.0 |
| **Sample Size** | **2,076** | | | **2,076** | | | |

TABLE E.9 *(continued)*

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SOURCE: Baseline and 12-, 30-, and 48-month follow-up interview data for those who completed 48-month interviews.

NOTE: The differences-in-means impact estimates are measured as the difference between the weighted means for program and control group members. Standard errors of these estimates account for design effects due to unequal weighting of the data and clustering caused by the selection of areas slated for in-person interviewing at baseline. The regression-adjusted impact estimates were obtained in two steps. First, separate regressions were estimated by gender. Each regression model included an indicator variable signifying whether the youth was in the program or control group and other control variables. In the second stage, a weighted average of the regression-adjusted impact estimates for each of gender group was calculated.

[a] Figures pertain to those without a high school credential at random assignment.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

errors differ by only about 2 percent. This finding contributes to our fear that the regression-adjusted approach may yield impact estimates that are slightly biased for the reasons discussed above.

### 4. Conclusions

On the basis of this analysis, we used the differences-in-means estimates as our benchmark estimates, for four main reasons. First, the gains in precision using the regression approach are small in general. In addition, because sample sizes are large, most impact estimates using the differences-in-means approach are fairly precise.

Second, because of the large sample sizes, there are very few differences in the average baseline characteristics of program research and control group members (as discussed in Schochet 1998a), so that controlling for these differences in a regression does not materially affect the estimates.

Third, we can fully account for the complex study design using the differences-in-means approach by using sample weights, so that we are confident that these estimates are unbiased and can be generalized to the study population (that is, are externally valid). As discussed, it is more difficult to account for the complex study design using the regression approach. The finding that the impact estimates using the two approaches typically differ more than the standard errors contributes to our concerns about the bias in the regression-adjusted estimates.

Finally, we can adjust for potential survey nonresponse bias using the differences-in-means approach by adjusting the weights. A similar approach in the regression context would create an even larger number of weighting cells, which would add to the estimation problem. Furthermore, adjusting for potential nonresponse bias using sample selection correction models would be difficult because we have no credible "instrumental" variables that are correlated with response status but uncorrelated with unobservable factors associated with the outcome measures.

We conclude by restating our finding that the two approaches yield very similar conclusions about the impacts of Job Corps for the full sample and for key youth subgroups. This result increases our confidence about the robustness of the impact findings.

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